PHILOSOPHICAL SOCIETY OF GLASGOW

THE MONTHLY

JOURNAL OF SCIENCE.

MAY, 1879.

I. THE SPIRITUAL IN ANIMALS.

CERTAIN writers, who have given up as untenable every other alleged ground of distinction between man and his "poor relations," take their final stand on the religious sentiment—on the recognition of a God and the belief in a future state: this, in their opinion, forms the final and insurmountable barrier. Before we can be expected to admit this assumed distinction it must be shown to our satisfaction, first, that all normal men are spontaneously worshippers; and, secondly, that all other animals are incapable of religious emotions. All other animals, we repeat; for if in any one species there can be found a trace of the "God-idea," then the boundary-line—whatever it may be worth—will run in an unwelcome direction, including that one species together with man, whilst excluding all the rest of the animal creation. Now where is the evidence that "brutes" are destitute of the religious sentiment? Simply nowhere. Who will dare to say that he has analysed all the emotions that pass through the mind of every animal? that he has looked upon the world with its eyes? Who can even guarantee that none of the actions of brutes have a religious significance? If we reflect how strangely varied are the devotional rites even of our own species, should we not pause before authoritatively deciding what must be the worship of other beings? The whole matter rests upon assumption. Man has complacently taken for granted that a "mere brute" must of necessity be without any trace of religion, and then, arguing in a circle, has elevated this imagined incapability of religion into a boundary-line between man and "mere brutes." We cannot recognise the validity of such reasoning. Lord Herbert of Cherbury, John Wesley, Meinhold, Quatrefages, and their followers, will possibly claim the right to assume the absence of the religious feeling in any animal till its presence shall have been formally demonstrated. We deny their claim. Before any attribute can be selected as distinguishing one body from another, its presence in the former and its absence in the latter must both be equally proved, not merely guessed. That the lower animals build churches and chapels, maintain a clergy, get up revival meetings, or distribute tracts, no one contends. But have they no dim but overshadowing consciousness of an Unseen Power behind, and above the visible and the tangible? Does not anything to them unwonted and unintelligible inspire them at times with awe, as contradistinguished from fear? Have they not—especially such as have been brought into human society—some vague idea of the last great change? Do we not find among them, in short, many indications of that lowest stage of religion—whether incipient or degraded it is not our business to inquire—which it is now the fashion to call fetichism? To us the cases lately observed and recorded by Mr. J. G. Romanes and others seem strongly to favour this view.

But it may again be doubted whether man gives everywhere more decided proofs of religious emotions. Unless this also is shown to be a rule without exception, the proposed boundary-line must again be declared at fault. To take only one instance: the Veddahs of Ceylon, as we are informed on good authority,* "have no knowledge of a God or of a future state; no temples, idols, prayers, or charms; no instincts of worship." Are they, therefore, men or brutes?

A kindred question here presents itself. Personal immortality—a second and prolonged, or rather never-ending, life—is claimed as a characteristic attribute of their own race by all the more cultivated portion of mankind, with the exception of a small minority of so-called materialists,† and is almost with one accord denied to brutes.

Many estimable persons will therefore be shocked at the declaration of the late Prof. Agassiz—himself certainly no materialist—that "most of the arguments for human immortality apply to other animals quite as forcibly." Take, for example, the doctrine of compensation, so much insisted on in the case of man. If a "land of the leal" hereafter be needed to atone for the inequalities of the present human life, the same plea may be urged also on behalf of the lower animals. How widely different, for example, may be the respective lots of horses. One may first see the light in a ducal stable, more comfortable than many a human dwelling; he is reared and trained with the utmost care and consideration, and fed on the choicest food suitable to his tastes; he becomes the pet of some lady of rank, and knows no harder task than to carry his mistress along Rotten Row. Another, from no fault of his own, is foaled in some rude shed, grows up amidst hunger, cruelty, and neglect, and before his bones and muscles are matured is doomed to drag a costermonger's cart—a similar fate to that which modern industrialism has prepared for the limbs and competitive examination for the brains of the young of our own species. Are not the respective destinies of these two horses almost as unequal as those of the financier and the farm-labourer?

---

* TENNENT, Ceylon, vol. ii., p. 442.

† An unhappily-chosen term. A materialist, correctly speaking, is one who in opposition to the idealist ascribes to matter substantive existence independent of the perceiving mind. In common usage it is applied to one who denies the existence of spiritual beings, and who ought rather to be styled an "apneumatist."

It may perhaps be contended that such inequalities are due not to Nature, but to man's interference. Let us, therefore, take an animal species not under special or direct human influence. Suppose any common female butterfly—say a *Vanessa Atalanta*—lays one hundred eggs. A portion of these never come to life at all, or are destroyed by a variety of causes before hatching. The remainder become caterpillars in due course, but very few ever reach maturity. Some are devoured by birds; others are pierced by ichneumons, and slowly consumed by parasitical larvæ. A few pass safely through their early stages of being, and come forth as butterflies. But here again one in the very outset of his career may perish in a spider's web; another may encounter its fate in the net of a collector, whilst the birds will not fail to secure an ample share. A very few only of the original hundred will live out their allotted span, find mates, and die finally a natural death. Is not here, also, a fair case made out for a hereafter to balance the capricious inequalities of the present?

In like manner we may deal with the other arguments for human immortality, and show their applicability to the lower animals. Thus orators and essayists love to enlarge on the unsatisfactory nature of human life. It is, they tell us, vain, empty, and incapable of filling our desires—a discovery which by the way is never made until health or strength, or at any rate elasticity and animal spirits, are declining. This, rightly or wrongly, is taken as a proof that we were intended for a higher destiny than this world can offer. But the lower animals likewise become life-weary; the gaiety and buoyancy of their earlier days vanishes; they, too, feel inclined to exclaim with the poet—

> "Life is had,
> And then we sigh and say, can this be all?"

The melancholy which all animals experience in the evening of their day is fundamentally the same. From the *vanitas vanitatum* of Solomon to the sadness of "a gib-cat, or a lugged bear, or an old lion," there is one continued series of phenomena, differing in degree, but alike in kind and in cause.

Have the lower animals, then, no right to say that life has palled on their taste—that this world does not satisfy their cravings? It is possible, indeed, that if a draught of some magic elixir could restore to them all they have left behind in the flight of years,—their youthful vigour and their flow of spirits,—this melancholy would disappear. But would not a similar change come over man?

Again we proclaim that our being is in part spiritual, on the very satisfactory ground that mere matter cannot be supposed to think, remember, reason, or even see or hear, and that spirit, being of its own essence indestructible, our immortality necessarily follows.* Be it so: the lower animals also see and hear, think, and remember. On our own showing, then, their nature, like our own, is partly spiritual, and like ourselves—if spirit is essentially indestructible—they may look forward to a future life. What that life may be for them we have not the faintest knowledge. It may have relations with that which awaits ourselves, or it may be totally distinct. We question if the more highly organised among them would find the prospect of annihilation satisfactory. To a transmigration of souls they would also have good right to object. To be perhaps born again as men of the nineteenth century—heirs to "competitive examination, sewage irrigation, and international arbitration," with the woman's rights movement looming in the future—they might, perhaps, feel it preferable to sleep on for ever beneath the shade of their good green woods.

---

* A strange writer in a somewhat heterodox journal, whilst assigning souls to animals, plants, and, if we understand him rightly, to minerals, and whilst fully admitting the immortality of the spiritual part of man, does not consider such part as indivisible. On the contrary, he maintains that the soul of every animal is an offset from that of its male parent. Every spermatozooid—or, as he holds, spermatozoon—contains a portion of the soul of the animal by which it is secreted. The female parent contributes merely the material part of the young animal. How all these points were ascertained it does not appear.

Will the future existence, either of man or brute, be more than merely linear, as at present? Who has not felt, at times, a craving for multipresence—a wish to see and enjoy at once what in this life we can only behold successively? As with sight, so also with all the other faculties of our being. Shall we be always restricted to the exercise of one only function at a time? Must the intellectual, the esthetic, the sensuous, and the moral phases of our being always merely relieve each other on duty? Of all the many limitations of our earth-life none is more irksome than this. Many a time when on a journey we have repined that we could not stay at some beautiful spot, and at the same time go on to other scenes not less lovely. The burden of a strange old song rings continually in our ears:—

> "For say, O why must the whitethorn fade
> Before the rose can bloom."

A bye-question here presents itself. The belief in ghosts—very widely spread among the human species—is frequently brought forward as a corroboration of the existence of unseen, probably immaterial, beings, and hence of the being of God, with whom man, in virtue of his spiritual nature, has an affinity. But in numerous accounts of the alleged appearances of ghosts, and indeed of phenomena not readily to be explained by known physical laws, we find it stated that dogs and other animals were seized with the utmost terror.* Hence it is but fair to argue that if these narratives contain an element of truth, then dogs, as well as men, have an affinity with the spiritual world, and are able to recognise the supernatural. If these stories are mere inventions, exaggerations, or distortions, or if the phenomena though faithfully reported admit of "natural" explanation, then they prove nothing as regards man.

---

* ENNEMOSER, History of Magic (HOWITT's version), pp. 355, 362, and 395.

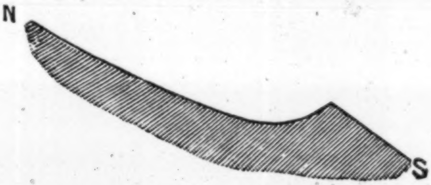
II. ANCIENT GLACIER ACTION IN THE PUNJAB:

WITH SPECIAL REFERENCE TO MR. MATTIEU WILLIAMS'S THEORY.

By Major H. A. TRACEY, Royal Artillery,
Fort Charles, Kinsale, Co. Cork.

TO enable my readers to trace on the map the country alluded to in the following lines, I would remind those who have not hitherto had their attention called to it that from Calcutta, on India's eastern border, to Peshawur, on its N. W. frontier, runs a road whose solid construction, bridges, and embankments rival Rome's magnificent works of the same kind. This Imperial monument of civilisation is about 1700 miles in length, and I, who have travelled more than 1000 miles along it, bear witness that it is but one of many of the grand Imperial highways of India. About 170 miles from the Peshawur end of this road lies the city of Rawul Pindi, and there branches off a road due north towards Cashmir, which in about 50 miles reaches the Cashmir frontier, just beyond the town of Murree.

I was quartered to the N.W. of Murree on a ridge whose geographical position is remarkable, and so narrow that at the part occupied by my hut I could throw out a tumblerful of water that would drain into the Indus from my N.W. windows, while from my opposite window the ground fell away and drained into the Jhelum. I was, in fact, on part of the actual water-shed that the Murree hills form between these rivers. It was on shaly rock we were situated, with occasional bold limestone bluffs. Our height above the sea was 8400 feet. In the autumn of 1876 I had marched from my eyrie beyond Murree to Rawul Pindi, and in the spring of 1877 was in camp at a spot 12 miles due south of it. I was then about 60 miles due south of Murree, but no longer in the "hills," being only about 1200 feet above the sea. While in camp there I received the number for April, 1877, of the "Quarterly Journal of Science," and read the article by Mr. Mattieu Williams, on the "Great Ice Age and Origin of the Till." I only wished I could have offered the author an Arab hospitality, and seated him at my tent-door, when I would have recounted to him some particulars of my route from Kyra Gully to the spot I was on that I think would have interested him. For instance, first day's march of only 8 miles:—Leave Kyra Gully 8400 feet; descend 1500 feet; ascend to Murree camping-ground, 7600 feet. Shale nearly the whole way, but limestone cropping out here and there. At Murree we got on to sandstone. Next day to Trete, 4000 feet, where the road and ground in section N. and S., goes thus:—



making a little basin: on the outer edges are built a few huts and houses: the whole basin very fertile. We were caught there by heavy rain. I, my men, my horses and mules, spent a miserable night there; every step in the soft red unctuous mud sunk deep; my tents bore its stains after repeated scrubbings, and the rough canvass cover of a valued folding-chair to this day bears witness to the tenacity of the clay with which that rock basin at Trete is filled, and which is unmistakably different to, and more tenacious than, any of the many fine specimens of mud and dirt passed on our way down.

Third day's march to Barakow, 1200 feet, and the plains of the Punjab are entered. The rapid change most oppressive for men and animals. The ground is strewn with great sandstone boulders. The stream that from Kyra Gully has run far below us on our right we cross, and find it swelling into a river, and at Barakow there are deep pools large enough to bathe with comfort in. Good shooting round Barakow, but country all sandstone boulders and scrub.

Fourth day, 17 miles to Rawul Pindi, 1700 feet. A long, level, monotonous tramp, stiflingly hot to us just fresh from higher levels, but delightfully cool to those who rode out to meet us and had spent their summer on the plains. Halfway we halted, where a bend of the river neared the road and a low ridge of sandstone rock ran from east to west, and on the northern side had been scooped out into a deep rock-pool, where lie plenty of fish, which, with the northerly aspect of the sloping rock, make this a favourite picnic spot from Pindi. The range of low sandstone hills run from E. to W., across the rapidly widening mouth of the valley we have descended, and repeat and lose themselves in the plain, 7 or 8 miles more of which bring us to Rawul Pindi, situated, you will remark, on ground that, rising imperceptibly, is still 500 feet *above* Barakow, and on whose northern side are extensive and dangerous bogs.

Fifth day's march. Pindi to Dargool. Still going due south and ground steadily rising; all hard baked clay, but within a mile of Pindi the ground dips again over another series of low undulating hill, this time not bare rock, but covered with rounded stones of every size from that of an ostrich's egg to a nutmeg. The occasional cuttings for the road show that many feet from the surface these ground and *striated* egg-shaped stones, cemented firmly by tough clay form the face of the country we are travelling over—officers dismount and walk by common consent, drivers take their beasts short by the head, and every one abuses India, the Punjab, and that particular bit between Pindi and Dargool heartily. But after a few miles the stones get smaller and smaller, the great cracks in the dry clay get deeper, the clay banks get higher and look more and more like fortifications, and then we almost suddenly descend into the valley of the stream that has followed us so long and see a level fertile plain in front of us, and the stream now swollen into a river, able to make itself respected, sweeps round under great jagged perpendicular clay walls, between 200 and 300 feet high on its left bank, and of unmistakably the same mixture as stained our tents so badly at Trete; while the right bank is for miles, level smiling young corn fields, among which stand our tents in bright relief near the river's edge.

Reading in that camp Mr. M. Williams's article, and recalling the character of the ground passed over in my recent marches down from the hills immediately north of me, I confess myself a complete convert of his views, and here record facts that seem to me to confirm them.

III. A PLAN FOR ESTABLISHING LIFE-SAVING AND SIGNAL STATIONS IN MID-OCEAN.

By Isaac P. Noyes, Washington, D.C.

AS navigation increases between the Old and New World, the necessity for life-saving stations in mid-ocean is more and more felt every year.

This has undoubtedly been a subject that in some way or other has presented itself to many minds; to some as only a passing thought, as quickly away as it came; to others a thought chiefly in connection with its seeming impracticability; while to a few it has been much dwelt upon, in hopes of solving it in some way or other that would be of practical value to mankind, either for the saving of life or for the interest of science. Had this problem, in all its various details, been easy to solve, so as to have become a practical reality, something ere this would have been done toward accomplishing it.

Of course the great impediment in the way is the enormous depth of water, making it, under any present known system, simply absurd to entertain thoughts of locating anything in the shape of a vessel or buoy that must necessarily require such an extensive cable to secure it in its place. There would be no trouble about the anchorage, or the vessel or buoy, in what was at the bottom or at the top, but simply in the connecting link between the two. "Simply" in this case though, like in many others, is the chief and almost impossible thing—that is, seemingly impossible up to this date. As we read the history of the world we see that safety-guards and institutions for the history of science are more and more established as the world progresses, and what at one age would not have been demanded, or even hardly thought of, in another is looked upon as an indispensable necessity. What is considered as impracticable in one age, in another or future age is smiled at and done apparently without the least possible effort, as though it was the most easy and natural thing imaginable.

Such a subject may prompt the question, Is it desirable even if practical? and perhaps many may scout the practicability of such an idea. When we read of some fearful accident or wreck at sea, where some noble ship containing hundreds of precious lives has gone down with all on board, or only a few left to tell the mournful tale, this question of life-saving stations far out away from land has presented itself in a very forcible manner, and the mind seems impatient and would leap at once to the rescue to prevent or lessen the fearfulness of such catastrophes. But no sooner is this thought through the brain than "How?" presents itself. How overcome this huge difficulty in the way? how make this *connecting link*, the cable, that shall secure the vessel or buoy to the anchorage at the bottom? After a number of years' thought on this subject a suggestion finally occurred to me whereby, I think, this connecting link may be made a practical thing, and no longer be in the way of the consummation of this auxiliary to the safety of ocean travel, and the means of taking regular mid-ocean observations the same as on land. For the cable proper I would not have anything unlike cables in general, yet would suggest a galvanised iron-wire cable similar to those used on large derricks, in place of the regular chain as employed by vessels for anchoring, as on the whole I think it would be stronger. But then the matter of the kind of cable in this connection just now is not of much importance, the principal point being now how to construct and lay it so that it will be a practical thing.

Some parties who have thought on this question have suggested the anchoring of an immense buoy, and making of this a huge store-ship, &c. In this place I would remark that I think that a buoy properly shaped and constructed would be better than a vessel, for the reason that the upper portion of the cable could be so arranged as not to interfere with passing vessels. Experts say that the trouble with the buoy and cable would be twofold: first, the great length of chain would require it to increase very fast in size as its distance increased from the bottom in order simply to have sufficient strength to hold itself up. Secondly, this great weight would necessitate a buoy or vessel of such huge proportions as to make the thing as a whole simply impracticable in the way of cost, &c.

Sometimes it may be best to have the strength of a thing concentrated in one support or bearing, but in general the more we divide it up the better; and this would seem to be the idea to follow out in this cable or connecting link.

There would be no trouble in constructing a number of small buoys. So in the place of one huge buoy, which experts say would have to be so large, I would have a number of smaller ones, and these I would have at intervals of about 250 feet, or thereabout. But the exact measurement of these intervals is of very little consequence at present; they may be more or less, and could easily be determined upon whenever it was proposed to carry the idea into effect, and would depend upon the size of the buoy, weight of cable adopted, &c.

This length, 250 feet, is simply taken in order to illustrate the plan with a little more facility, as most people can understand such things by illustration better than by abstract or technical terms. The depth of the Atlantic Ocean where such anchorages would be required is from 10,000 to 15,000 feet. At 250 feet for a section, this would make from twenty to thirty sections, requiring twenty to thirty buoys. If each buoy had sufficient bearing capacity to support its section of cable, it will readily be seen that the cable will stand as to strength on the same relative proportion and basis as any ordinary cable of 250 feet; so that if the anchor was in water 3 miles deep the strength of chain or cable would be intact for the purpose of holding the vessel or buoy at the surface, as though at any ordinary anchorage.

Many may ask, how are we to get these buoys—all strung, as it were, on this cable—into position? This, at first, seemed as insurmountable as the cable itself, but now it seems the easiest thing imaginable, and, in fact, not more difficult than it would be to effect any deep anchorage. Let the cable be constructed with the buoys all attached at their regular intervals, and in this manner towed to their respective grounds, soundings of which should be taken in advance, in order to determine the necessary length of cable, allowance to be made for the angle at which the cable would lie in the water. When this has been accomplished, secure the anchor and let go, and like any other anchor there would be no trouble in its finding its way to the bottom and taking hold.

When located, these buoys or stations should be manned much after the manner of lightships and life-saving stations, combined with lights, signals, stores, and life-saving implements; and, in addition to this, have regular signal officers stationed upon them, with established communications with the head-quarters on shore *via* the ocean telegraph. Then we could begin to trace storms upon the ocean as well as upon the land, and if for no other purpose it would seem that these stations would pay for themselves.

After having constructed the stations and suggested what could be accomplished by them, the practical mind will certainly ask how will we pay for them? Even if good, there must be a great many of them in order to have them of any practical value. I propose to follow up the same idea as in regard to the construction—divide up the cost between the nations. All the civilised people of the world are interested in the safety of the travel between the Old World and the New, and it would seem should likewise be interested in the attempt to trace the line of a storm on the ocean as well as on the land, for only thereby can we become as familiar as we should be with the weather of the whole globe.

The great powers, like the United States, England, France, Germany, Russia, &c., should each construct and maintain a certain proportion; and the smaller and more distant powers, like Holland, Italy, Turkey, &c., a smaller proportion each. In order to stretch across the ocean between America and Europe, a contribution of two from each of the larger powers and one from each of the smaller powers would locate them within the comparatively short distance of 150 to 200 miles of each other. We cannot locate ocean accidents or tell just where storms may happen, and then place our buoys or vessels; neither is there an attempt to do this in our life-saving and weather stations on land: that would be simply impossible. It is not proposed either at present to be able to get them close enough to be in the neighbourhood of every accident or on the line of every storm; yet if placed at these intervals I think that they would pay for themselves in the good they would secure to humanity. We read of mighty works done in the past, but when we come to look at them carefully we find them executed by unwilling hands,—a serf class,—all for the mere vanity of those in power, and not for any real good to mankind. Somehow or other the world has always spoken of the present age in a mere hard mechanical way. We have the Stone, Brass, Iron Age, &c., though occasionally we have the more intellectual designation of "Age of Reason," yet I think there is another name by which this age should be known, "Age of Humanity." No age in the world has done so much for humanity at large; sure there are some minor exceptions, but then all ages have had these more or less. I think that we have the least bad and the most good; so, in respect to the numerous things that are done purely out of brotherly love, I think it full time that we call this the "Age of Humanity," and I hope that the benefactors of mankind may go on and never stop, but continue to extend their genial influence from age to age. Here, in this ocean life-saving and signal service, I think that there is an opportunity for some of our well-disposed millionares like Peabody, Lick, Cornell, and others of their kind, to establish one of these stations in the ocean. I doubt if their money could be put to any better purpose than in helping carry out some such plan for the benefit of Science, and for the additional safety of those who go down to the sea in ships."—*Kansas City Review of Science and Industry.*

IV. AN HONEST CASHIER.

IT is one of the curious things in nature that the animals nearest to man in the order of development are of little or no use to him industrially. There has never been a time when strong races of men have not compelled their weaker brothers to work for them. But, barring the showman and the organ grinder, the meanest of men have not been able to subjugate or enslave their simian relatives. An ancient Arabic proverb accounts for the freedom of apes by the fact that they shrewdly refuse to talk: "well they know that were they to speak they would be made to work; so they wisely hold their tongues."

The proverbial prudence of the monkey appears to fail in a measure, however, in the land of the white elephant. An Austrian resident at the Court of Siam reports that in that country the monkey is trained to fish for crabs with his tail, as comical a pursuit as can well be imagined, except, perhaps, for the worthy and intelligent ape engaged in it, who sometimes gets a "bite" from a monster crab that he is totally unable to land, and falls a victim to the superior weight of his Cancer Ferox, who drags him into the water, drowns, and finally devours him. The Siamese ape is also stated to be in great request among native merchants as a cashier in their counting-houses. Vast quantities of base coin obtain circulation in Siam, and the faculty of discrimination between good money and bad would appear to be possessed by these gifted monkeys in such an extraordinary degree of development that no mere human being, however carefully trained, can compete with them. The cashier ape meditatively puts into his mouth each coin presented to him in business payments, and tests it with grave deliberation. If it be genuine he hands it over to his master. If it be counterfeit, he sets it down on the counter before him with a solemn grimace of displeasure. His method of testing is regarded in commercial circles as infallible; and, as a matter of fact, his decision is uniformly accepted by all parties interested in the transaction. But, though a true and invaluable servant to his own particular master, it seems that his moral character is not altogether irreproachable. His deplorable passion for fruit renders him the terror of Siamese market gardeners, who find brute force inadequate to restrain him from visiting their orchards, and therefore have recourse to divers and sundry stratagems, one of which is reported to be as successful as it is certainly ingenious. A specially active and enterprising ape is captured and carefully sewed up in the skin of a tiger cat. He is then turned loose in the orchard of his predilection, and straightway clambers, as well as he may, incumbered by an unfamiliar garment, into the branches of a fruit tree among his unclothed fellows. Scarcely do these latter set eyes upon him with all his feline terrors thick upon him, when a dreadful panic strikes them, and they scramble away with piercing screeches and agonised chatterings. Never more do they return to an orchard which they believe to be infested by the deadliest enemy of their race. The startling intelligence is rapidly disseminated throughout the monkey society of the neighbourhood, and the wily gardener enjoys an absolute immunity from depredation for ever afterwards, for the very thought of a tiger cat appals the simian soul, and doubtless the tale of "the awful apparition in Ting-tse's orchard" is handed down in quadrumanous families from generation to generation.—*Scientific American*.

V. OPIUM SMOKING AMONG THE CELESTIALS.

By Rich V. Mattison, Ph.G., M.D.

AS one passes through the Chinese quarter of San Francisco he cannot help being sharply impressed with the immense traffic in an article which is seemingly part of the very life necessities of this curious people. We seem scarcely to pass a shop, whether devoted to the sale of clothing or drugs or groceries, but what we find a notable proportion of the business to consist in the sale of opium. We pass the shop of the merchant, and while one assistant is counting out the gold for a bill of exchange on the Flowery Kingdom, we see another weighing carefully a small portion of the much coveted drug. The jeweller, surrounded by the precious bracelets of nephrite and phrenite, lays aside for the moment the curious golden circlet he is filing, to catch up the balance and poise upon the pan the little horn cup a moment, and again return to his employment. The grocer, surrounded by the many dainties of Mongolian gastronomy, stands under the rows of varnished fowls, balance in hand, dispensing the drug with the most imperturbable gravity and solemnity. As we stand by the half-open doorway on one of those beautiful summer evenings so common to Pacific climes, a young celestial enters the shop to return in a moment laden with his store of dreamy forgetfulness, the absorption of which transports him, in imagination, to his native land, where riding in a gorgeous palanquin, with maidens to fan him and coolies to fly at his slightest wish, he passes into his dwelling by Kin-Sha-Kiang, or the river of the golden sands, where his wife, with the feet of a mouse, brings his tea in golden cups, and so he passes into the arms of Morpheus (or Morphia's meconic embrace), his couch covered with scarlet and silken curtains with fringes of golden strands, only to awake finding himself lying coiled up on a hard board shelf covered with matting, his head upon a block; for now transported by the magic lamp of a private detective we are in the classic precincts of an opium den. To reach it we have passed through many dark, subterranean alleys, through courts of filth and squalor and wretchedness to any other than Mongolian eyes. On either side of the room, which is about sixteen feet square, are accommodations for twenty or more smokers—shelves rising in tiers like the bunks of a steamer's cabin. In the centre is a small table covered with the shells, bowls, cups, lamps and other paraphernalia of a first-class opium den, sustained by liberal patronage. It was early evening, scarcely midnight, and at our right inclined a strong, sleek, almond-eyed native of a foreign land, well known to our guide as one of the most inveterate smokers of the city. Immediately in front of him was a small saucer filled with lamp oil, and inverted over it was a tumbler in the bottom of which (or the apex as it was placed) a small hole was drilled, through which protruded a piece of wick—this being lighted constituted the lamp. By its side lay an oyster-shell containing a quantity of a dark coloured extract, and on either side a long wire exactly like the knitting needles of our grandmothers, excepting that one extremity ends in a small spoon. The pipe is naturally of interest. The most usual style is that having a shaft of bamboo, resembling somewhat a flute pierced laterally at each extremity, at one of which is fitted a small metal cup in which to receive the bowl of the pipe proper. This bowl is of earthenware or metal, and is about 3½ inches in diameter, convex on both its upper and lower surfaces, the latter ending in a tube of half an inch in diameter and of similar length; this fits into the metal cup of the bamboo shaft. The upper convex surface is pierced in the centre with a metal tube, having a funnel-shaped aperture about one thirty-second of an inch in diameter. The cavity of the bowl, as here exhibited, is of a capacity of nearly a hundred cubic centimetres.

The pipe is filled by taking up on the apex of one of the knitting needles a small portion of the extract, usually from 2 to 5 grains, and holding it momentarily in the flame of the lamp, rotating the needle dexterously meanwhile, then withdrawing it only to repeat the same operation until the extract is dried to light brown colour and of just such consistency that it sticks to the pipe, when, with a dexterous twist, the point of the needle is inserted into the apex opening of the upper convex surface of the bowl, and the needle instantly withdrawn by a rotary motion. This manœuvre places the extract in the shape of an inverted pyramid, with a central opening communicating with the orifice leading into the cavity of the bowl. It is during this evaporation of the extract over the lamp that the cultivated smoker judges of the quality of the opium. If it bubbles up to that delicate shade of light brown, and at the same time gives off the peculiar odour so characteristic to the trained olfactory nerve bulbs of the Mongolian smoker, then is he satisfied of the quality of the extract purchased.

The bowl being filled, it is inverted over the flame of the lamp at an angle of about 45°, and the volatilising narcotic rapidly drawn by a few strong inspirations into the body of the pipe, and so on into the pulmonary cavities. The inspiration thus made is peculiar; it is not only buccal, but more strongly pulmonary. The inspiration is slow and deep and prolonged, until the chest is filled with the narcotic vapour, and expiration then occurs with the mouth closed, and the inspired smoke issuing slowly through the nostrils.

The quantity smoked varies greatly with different smokers, it varying from 30 grains to upwards of 300 or 400 grains at one sitting, this being from 10 to 100 pipefuls of the extract. The whole process is done in the most methodical manner. The veriest coolie or wealthiest merchant proceeds to arrange his lamp and extract and pipe with a solemn gravity ludicrous to behold. In this grave affair of state, perhaps five, ten, or even fifteen minutes are consumed; then follows an interval of placidity in those unaccustomed to smoking, to be followed by another and another smoke. But we begin to feel almost like opium smokers ourselves breathing so long the atmosphere De Quincy called "the mephitic regions of carbonic acid gas," so we ascend to the upper regions of the street, and realise, as we never have before, the fresh, invigorating influence of the brisk midnight air of the ocean coast.

The above is taken from the "American Journal of Pharmacy." The effects of opium-smoking have been recently tested by N. von Miklucho Maclay during his stay in Hong Kong. The experiment was made at the Chinese Club, where every convenience for smoking opium is to be found. Dr. Clouth, of Hong Kong, took the necessary observations, and his notes are summarised as follows in the "Chemist and Druggist":—Herr Maclay was in normal health, and had fasted 18 hours before commencing the experiment. He had never smoked tobacco. Twenty-seven pipes, equivalent to 107 grs. of the opium used by the Chinese, were smoked in two and three-quarter hours, at tolerably regular intervals. The third removed the feeling of hunger caused by his long fast, and his pulse rose from 72 to 80. The fourth and fifth caused slight heaviness and desire for sleep, but there was no hesitation in giving correct answers, though he could not guide himself about the room. After the seventh pipe the pulse fell to 70. The twelfth pipe was followed by singing in the ears, and after the thirteenth he laughed heartily, though without any cause that he can remember. Questions asked at this time were answered only after a pause, and not always correctly. He had for some time ceased to be conscious of his actions. After the twenty-fifth pipe, questions asked in a loud tone were not answered. After the last pipe had been smoked he remarked, "I do not hear well." Forty minutes later there was a slight return of consciousness, and he said, "I am quite bewildered. May I smoke some more? Is the man with the pipe gone already?" Fifteen minutes later (4.55 P.M.) he was able to go home, and then retired to bed. He woke the next morning at 3 A.M. and made a hearty meal, after his fast of 33 hours. During the next day he felt as if he had bees in a great hollow in his head, as well as a slight headache. The organs of locomotion were first affected, next came sight and hearing, but Herr Maclay is very positive that there were no dreams, hallucinations, or visions of any sort whatever.

VI. IS ORGANIC VARIATION FORTUITOUS?*

ADMITTING the broad fact that the organic world, like the organic individual, has been produced not by any sudden and arbitrary intervention of supernatural power, but by a gradual course of Evolution, the question remains—What is the efficient cause of such Evolution? Are species, as we find them, due to the mere accumulation of fortuitous changes through an almost infinite succession of ages? Or can we trace "in the inner domain of life" other and more powerful factors than Natural Selection? A profound thinker suggests as such agencies "Habit and Intelligence." By Habit he understands "that law in virtue of which all the actions and the characters of living beings tend to repeat and to perpetuate themselves not only in the individual, but in its offspring. Mr. Murphy's "Habit," therefore, includes what is commonly known as heredity. He considers intelligence "an attribute of all living beings, and coextensive with life." But by intelligence he means "not only the conscious intelligence of the mind, but also the organising intelligence which adapts the eye for seeing, the ear for hearing, and every other part of the organism for its work." It will easily be seen that the inquiry thus opened must be to no small extent a critique of the hypothesis commonly known as Darwinism. But from the majority of writers who have undertaken such a task Mr. Murphy differs most favourably. He is a decided Evolutionist, agreeing with Darwin "in the belief that all species have been derived by descent with modification, probably from one, certainly from a few original germs." He admits Natural Selection to be "a really operative agent—a *vera causa*," but he differs from Darwin and from his direct disciples by contending—and in our opinion with great cogency—that "the process of

---

* Habit and Intelligence: a Series of Essays on the Laws of Life and Mind. By J. J. MURPHY. London: Macmillan and Co.

Evolution proves the agency of an Intelligent Power, acting through and controlling the unintelligent forces of Habit and Variation, just as all the vital forces act through and control the inorganic ones."

But though thus differing from Mr. Darwin on a fundamental point, our author never attempts to undervalue his high authority and the services he has rendered to Natural Science. Such a passage as the following stands by no means singly in the present work:—"Much is to be said for this theory, as for everything that Darwin has ever proposed." Nor have we any imputation of motives. In this respect Mr. Murphy stands out in contrast, not merely to a number of lesser luminaries, but to a most distinguished biologist whose views on the Darwinian question in many points approximate to his own. In further contradistinction to certain writers on Evolution the author is not a mere man of culture—an outsider who has "read up" the subject, and who then assumes to teach when scarcely qualified to be a learner. On the contrary, he tells us that for ten years he has been thinking, reading, and conversing on the subjects discussed in this volume, and after a careful examination we see every reason to accept his avowal. Of one of the non-biological critics of Darwinism Mr. Murphy indeed speaks, in our opinion, too favourably. Referring to a somewhat over-rated article in the "North British Review" (June, 1867) he writes—"This is a good instance of the service that an able man may do to a science which is not his own, and of which he does not know the details." Yet in an earlier passage of his work he quotes from the very same article an objection against Evolution almost childish in its character, and based on utter ignorance of the subject under discussion. Concerning this argument Mr. Murphy very justly remarks that it is "one which could not have occurred to a naturalist."

The grounds advanced against the sufficiency of Natural Selection, to account for the origin and characteristics of species, vary greatly in their value. One of the most weighty, recently insisted upon in the "Quarterly Journal of Science" (vol. viii., p. 453), must be sought in the fortuitous destruction which takes place among young animals, quite irrespective of their greater or less "fitness" for survival. Such destruction is fully admitted by Mr. Darwin himself. This difficulty, together with that pointed out by Prof. Tait, may be summarised as follows:—"The final establishment of the superior type is dependent at each step upon three accidents. First, the accident of an individual sort or variety better adapted to the surrounding conditions than the then prevailing type; secondly, the accident that this superior animal escapes destruction before it has had time to transmit its qualities; and thirdly, the accident that it breeds with another specimen good enough not to neutralise the superior quailties of its mate."

A still more important argument is drawn from structure in advance of function. It is obvious that Natural Selection can only preserve such varieties as are immediately and directly useful to the organism in which they occur. Yet, as Mr. Murphy shows by a number of well-selected examples, there are cases "where structure has been laid down as a preparation for function before the function could be brought into action, as truly as the shipwright when he lays the keel on the land intends the future ship to float on the water." Thus in the metamorphosis of Crustaceans one particular stage—the so-called *Zoea* phase—is characterised by a prolonged abdomen calculated for future utility, though for the time being rather an incumbrance. Elsewhere the author asks, of what use can a dorsal groove and an incipient cartilaginous band be to Ascidian larvæ? Yet, if we regard these creatures as representing the common ancestors of the Vertebrates and Ascidians, we have here the first outlines of the vertebral column essential to the former. The transition from the swim-bladder to the lungs, from the fins to legs with toes, and from the reptile's fore leg to the bird's wing, are also, on the hypothesis of Natural Selection, beset with difficulties. We can scarcely imagine any intermediate stages between the two which would not be useless, even if not positively injurious, to the animal in which they might occur. Mr. Murphy expresses the opinion that this argument is new, and that, if taken up by thorough anatomists and embryologists, "instances of structure in anticipation of function might be found everywhere in the organic world."*

The phenomena of mimetism, or organic mimicry, have generally been considered as affording strong evidence in favour of the Darwinian view. But it is difficult to understand how the first slight variations in the direction of approximating a defenceless animal to some formidable species could be of such essential utility to the former as to ensure its preservation by Natural Selection. A further very formidable difficulty is that the mimicking form is—and if the mimetism is to be useful must be—rare. Now, according to hypothesis, it is only very numerous species which are likely to yield individuals possessing the required variations. Another difficulty is to be found in the metamorphosis of certain Dipterous insects. Instead of the tissues of the larva being transformed into those of the perfect insect, they seem to be melted down, except at certain spots, into a semi-liquid substance, from which the tissues of the perfect insect are afterwards re-developed. Mr. Mivart bases on this exceptional fact an argument against the all-sufficiency of Natural Selection.

---

* A somewhat similar argument, at least as far as concerns the dangers of intermediate stages, may be found in the "Quarterly Journal of Science" (vol. v., p. 329). The instance there given is, however, much less happy and conclusive than the cases adduced by Mr. Murphy.

To the contention that geological time is too short for Darwin's theory, few geologists and biologists will attach much importance. The very discrepancies in the calculations of the mathematicians and physicists who have undertaken to fix a maximum limit—discrepancies which are in the proportion of 1 to 40—might provoke a smile.

From all these considerations, and from many others which we cannot here particularise, Mr. Murphy concludes that organic species and their peculiarities are due not alone to the accumulation of fortuitous variations, but to Intelligence. Still he does not hold that the intelligence which directs the formation of organic structures is necessarily conscious, nor that such structures and their adaptations are the direct work of Creative Wisdom: he considers it "more reasonable to believe that organic progress has been effected not by a fresh exertion of Creative Power at every one of the innumerable stages, but by a principle of Intelligence which guides all organic formation and all motor instincts, and finally attains to consciousness in the brains of the higher animals, and to self-consciousness in the brain of Man." He thus, equally with Darwin, avoids making God responsible for the imperfections of the organic world, for the existence of parasitic worms, &c., which on the hypothesis of the Old Natural History "can only be regarded as instruments of torture devised by the Creator, and whose existence no writer of Bridgewater Treatises has yet even attempted to reconcile with His infinite wisdom and benevolence."

An important consideration, which would well repay special notice did space allow, is that the constructive instincts of social insects—bees, wasps, and ants—cannot be inherited in the direct line, because these instincts are possessed by the workers alone, which are non-reproductive. This, however, is only partially correct. Female wasps, humble-bees, &c., especially at the first establishment of a new colony, enjoy none of the exemption from labour which falls to the lot of the queen hive-bee and of female ants.

Mr. Murphy remarks that "neither association nor the principle of imitation will account for the child's use of the word I," and quotes Ferrier's opinion that the use of this pronoun proves not only intelligence, but self-consciousness, which appears to belong to man alone. We have certainly met with no instance of a parrot or other talking bird speaking of itself as "I." But it is interesting to note that young children, before learning to use the personal pronoun, speak of themselves by the same name which is applied to them by others. Just as the parrot which is addressed as Polly connects that word with itself, and if desirous of anything says, *e.g.*, "Give Polly a bit," so the young child addressed as "Baby" speaks of itself as "Baby." This is additional evidence that a child at a certain early age is mentally on a level with the mature parrot, and shows that the difference between man and beast is one of degree rather than of kind.

Incidentally we note that the author considers it possible, "though to our faculties not conceivable, that the number of dimensions in Space may be infinite, though we exist and move in only three." The discussion of this interesting speculation, which seems gradually dawning on the human mind, must be left to mathematicians.

On p. 571 of Mr. Murphy's work we find an assertion with which we are unable, without certain reservations, to agree. We read that "the more highly organised among organisms grow to the largest size and live the longest." We fully admit that the Vertebrata are as a class more highly organised, longer-lived, and larger than the Evertebrates; but the birds, and probably reptiles and fishes, though less highly organised than the mammals, appear to live longer; and even within the mammalian circle, man and the anthropoid apes, though unquestionably the most highly organised, take but an intermediate rank in stature.

Dr. Beale's argument—not against Darwinism, but against Evolution in general—that the microscopic and chemical characters of species are less variable than their external features, requires an amount of attention which it has not yet received.* Mr. Murphy very properly declines to pass any definitive judgment on the question.

It will not be irrelevent for us to pronounce our conviction that "Habit and Intelligence" will amply repay a close and critical study, and that the eighteenth chapter in particular points the way to a field of research not to be neglected.

---

* BEALE's edition of TODD and BOWMAN's Physiology, p. 41.

VII. THE SCIENCE OF AGRICULTURE.

It is now, we suppose, universally admitted that agriculture, in order to be progressive and profitable, must be conducted on scientific principles. To attain this end and to meet the demands of an increasing population the United States Government has for a long time included a Department of Agriculture, the main object of which is "the introduction of all the productions of the earth that can be grown in any part of the country, and to encourage by every means that diversity of production which is at once the safety and the wealth of the nation." From the Prefatory Report of the Commissioner of Agriculture for 1878 we learn that experiments were made during last season on the growth of different varieties of sugar cane. As far as the experiments go a variety of cane from Jamaica, called the "Salangore," is shown to be worthy of extensive introduction and trial. The attention of the Commissioner has, however, of late been more especially given to the question of producing large supplies of sugar from sorghum and maize. He procured as much as possible of the pure well-cured seed of a variety of sorghum called the "Minnesota Early Amber," and distributed the same in every Congressional district in the United States. The results of this distribution have been most favourable, and the variety has yielded everywhere a large amount of rich saccharine juice, which under proper treatment gives excellent sugar and syrup, the yield being from 120 to 250 gallons of heavy syrup to the acre. It is proposed another season to make experiments with the different varieties of maize and sorghum, and to ascertain the different modes of cultivation and the stage of growth at which the production of sugar is at its maximum, in order that with as little delay as possible the country may be prepared with all necessary data to enter intelligently upon this new industry.

The great drawback to the work of the Department seems to be the want of a larger chemical laboratory with a sufficient appropriation to meet the expenses of the additional force that will be necessary to carry forward investigations on a larger scale than the present laboratory and appliances will permit. With the facilities of the existing laboratory much information which skilful chemical analysis can only determine is, remarks the Commissioner, necessarily withheld from the farmer and the manufacturer. The report contains extracts from letters from prominent agriculturists in the United States, all of whom testify to the fact that the agricultural interests of the country would be greatly advanced by a more thorough analysis than has yet been made of the grains, grasses, and edible roots, in order to determine the exact value of each in the production of milk, beef, and fibre, or muscular power.

The English Government would do well to follow the lead of the American Government by establishing experimental agricultural stations in various parts of the kingdom with properly furnished laboratories and experienced chemists. It is impossible to over estimate the benefits to agriculture in England resulting from the scientific experiments made on a large practical scale by Messrs. Lawes and Gilbert at Rothamsted, but we contend that such work ought not be left entirely to private enterprise.

In the preface to the English translation of his Lectures on Chemical Manures* at the Experimental Field at Vincennes, M. Georges Ville remarks that it is important that both England and France should be alive to the fact that the agricultural crisis from which both countries are now suffering, as well as the more serious troubles which threaten civilised nations, are only the prelude to the economic struggle between the Old World, bound in the trammels of tradition, and the New World, pressing onward free and unrestrained in the path of progress.

At a period when the means of communication had not reached the development which they have since acquired, the home markets provided certain and easy outlets for agricultural produce. But at the present time, with free trade and the facilities of transport, farmers are compelled to compete in our own markets with all the world. In order that the struggle may be possible and remunerative, it is absolutely necessary that crops of every kind should be increased to their utmost possible limit. The traditions of the past are not sufficient for the necessities of the present. We want more rapid, more economical, and more powerful processes. The agriculturist used to divide the land into two nearly equal parts, setting one aside for grazing purposes, or for growing forage plants, and reserving the other for cereal crops, which was equal to asserting that in order to grow cereals there must be meadow land, cattle, and manure. Instead, however, of growing meat in order to have corn, he must grow corn for profit's sake in the first place, and afterwards for straw, cattle-feeding, and manure.

---

* On Artificial Manures, their Chemical Selection and Scientific Application to Agriculture. A Series of Lectures given at the Experimental Farm at Vincennes during 1867 and 1874-5. By M. GEORGES VILLE. Translated and Edited by WILLIAM CROOKES, F.R.S. Illustrated with Thirty-one Engravings. London: Longmans, Green, and Co. 1879.

The object of the farmer, then, should be not to produce manure, but to manure his land more abundantly than formerly. No matter what may be the material he employs, whether it be farmyard or chemical manures, used either together or separately, he must somehow or other give back to the soil a larger amount of fertilising material than that lost by the growth of the crops. In the cultivation of the soil increase of production depends less on the worker and on the quality of the tools which he employs than upon the quantity of fertilising materials which he has at his disposal. According to M. Ville the only way to do this is to employ chemical manures, and to prove his assertion, to show that with chemical manures large crops may be quickly obtained from the most barren lands, he refers among others to an experiment carried out by M. Ponsard, President of the Agricultural Committee of Omey in Champagne, on a piece of waste land in one of the most barren districts of a proverbially barren portion of that province. M. Ponsard manured one half of the ground with about 32 tons of farmyard manure per acre, and the other with about half a ton of chemical manure per acre. With the farm manure he obtained 14 bushels of wheat, whereas with chemical manure the land yielded about 30 bushels, there being a loss of £19 in the former case and a gain of £17 in the latter.

Similar experiments have been made with beet-root, potatoes, sugar-cane, &c., and in each case the results have been in favour of the chemical manure. In fact, by varying the quantity of the ingredients entering into the composition of chemical manure, so as to suit the requirements of each class of plants, the work of vegetation may be regulated almost like a machine, the usefulness of which is in proportion to the fuel it consumes. The first point is to discover the degree of richness of the natural soil, and then to ascertain the dominant constituent of each plant. Plants are divided into three categories—first, those in which nitrogenous matter is the dominant constituent, such as cereals, hemp, colza, beet-root, and general garden stuff. The second group, in which calcic phosphate preponderates, comprises maize, sugar-cane, Jerusalem artichokes, turnips, and sorghum. The third group includes leguminous plants, such as clover, sainfoin, lucerne, potatoes, and vines, and in these potash is the dominant ingredient.

Now what M. Ville terms "normal manures" contain calcic phosphate, potash, lime, and nitrogenous matter, differing only in the respective proportions of these four substances. By varying, therefore, their relative proportion according to the necessities of the particular plants for which they are required, the principle of dominant constituents can be applied to every possible condition which may arise, thereby meeting the requirements and advancing the interests of every description of farming.

It is not necessary to restore to the soil, weight for weight, constituent for constituent, all that is taken from it, but the four constituents named above are essential, and must always be added. Analyses of farmyard manure show that it contains the four constituents which it is essential to restore to the soil, but it also contains carbon, hydrogen, and oxygen; also sodic chloride, magnesia, soda, silica, ferric oxide, &c., all of which are abundantly contained in the poorest soils, and which do not therefore increase the value of manure. Farmyard manure therefore owes all its efficacy to the four essential constituents mentioned above. But we have just shown that each of these constituents with regard to the three others fulfils functions that are in turn subordinate or predominant, according to the nature of the plants to be grown; with farmyard manure, however, there is no possible division; its composition cannot be varied. The only alternative, then, is to use it in conjunction with chemical manures. In practice the quantity of farmyard manure usually applied to an acre of land is, we believe, from 16 to 20 tons, in which quantity the four essential constituents form only about a fortieth of the whole mass. Their proportions are as follow:—

Nitrogen	181 lbs. per acre.
Potash	164 ,,
Phosphoric acid ...	98 ,,
Lime	352 ,,

To place the land under the proper conditions for high cultivation the amount of the fertilising substance in the farmyard manure must be at least doubled by means of chemical manures, and in the case of each particular plant it is necessary to concentrate that chemical agent which is especially favourable to its growth. It must also be remembered that one-third of the nitrogen is lost to the soil on account of the decomposition which the manure must first undergo before it can exercise its action.

M. Ville strongly advocates the foundation of experimental fields. They are, he affirms, the only reliable method of ascertaining with certainty the composition of the soil with respect to the requirements of agriculture. A piece of land should if possible be selected which in its physical nature and degree of fertility represents the average quality of the land that is to be cultivated. For a newly-worked farm the field should consist of twenty plots, each containing about four poles, arranged in two parallel rows of ten plots each. The first row should be devoted to the cultivation of wheat, and the second to that of beet-root or potatoes, according to the climate and the wants of the district. The wheat furnishes indications of the richness of the superficial layers of the soil, and the beet-root of the deeper layers. Full instructions are given respecting the manuring of the several plots. M. Ville also gives directions for establishing experimental fields for agricultural colleges, societies, and for elementary schools. For the latter the plots should be about eleven yards square. By carrying out the advocated system of manuring it will be conclusively established that it is possible to farm without using farmyard manure; that a manure can be and is composed which more than takes its place, and that the action of animal manure is intensified by the addition of chemical manure.

VIII. MEASUREMENT OF POWDER PRESSURES IN CANNON BY MEANS OF THE REGISTERED COMPRESSION OF OIL.

THE determination of the pressure exerted by the elastic gases produced by the combustion of gunpowder is an element of the first importance in the theory and practice of gunnery, and a measure of this pressure is indispensable in all mathematical computations of the effects of gunpowder on the projectile or on the gun.

In 1855, Dr. W. E. Woodbridge conducted some experiments at Washington Arsenal with the view of measuring the powder pressures of fired gunpowder. The results of these experiments were not published by the U.S. Ordnance Department until November, 1878, when some additional experiments were recorded by the author.

It was proposed in these experiments to ascertain the pressure of the gases evolved by the combustion of gunpowder, by including in the cavity within which the pressure should be restrained a piezometer, which, by registering the compression of a liquid contained within it, should afford an indication of the pressure to which it had been exposed.

This instrument, as employed in these experiments, is a small cylindrical vessel of steel, inclosing a quantity of oil destined to receive the pressure of the fluid by which it may be surrounded, through the medium of a piston, which is carried inward a distance proportional to the amount of compression. To the piston is attached a stem of wire, extending inward, on one side of which a fine point is made to press, inscribing, when the piston is moved, a line on the stem equal in length to the extent of its motion.

In preparing the piezometer for an experiment, two items are to be specially observed: it must contain no air, and the "setting," or adjustment of the quantity of oil contained, must be done at the precise temperature the instrument is to have at the moment of firing.

The procedure is as follows:—All the parts are first oiled over their whole surfaces. The adjusting screw is inserted into the body of the instrument, which is then set upright in a socket attached to the middle of a small pan intended to catch any overflow, and is nearly or quite filled with oil, which should be made to flow down the side of the cavity rather than in a stream. The support of the marking-point, quite clean but covered with oil, is now screwed into its place, with the aid of a special implement, described. When this is withdrawn, it will be necessary to replace the oil caused to overflow by its insertion. The barrel is now slowly put in its place and screwed firmly down. The hole in the piston for receiving the stem is filled with oil, the stem screwed in, and the piston inserted in the barrel. The adjusting-screw is loosened a little, permitting the piston to be pressed just below the top of the barrel, and again tightened. The next step is to bring the instrument and its contents to the setting temperature. For this purpose a water-bath (a common wooden pail) was provided; a narrow tin cup, deeper than the bath, and weighted at the bottom so as to stand upright within it; and a pair of wooden pinchers for handling the piezometer, which instrument could be inserted in them in such a way as to be nearly enveloped and yet to leave the adjusting-screw and piston readily accessible. The piezometer, seized in the pinchers, is placed at the bottom of the cup in company with the tools to be used in setting it, and is covered with a loose wad of cotton. The cup is set in the middle of the bath and surrounded with water, kept as nearly as possible at the desired temperature, for a sufficient time to impart, as nearly as appreciable, the same to the instrument. It is then withdrawn, the screw loosened, the piston depressed a little to a regulated depth with a special tool, the screw tightened, and the piston rotated a few degrees, which completes the setting. The object of this last movement is to inscribe a transverse line on the stem, affording a starting point in measuring the length of the stroke.

Small changes of temperature after the instrument is set are of no consequence, as the oil will of course return to the same volume, and the piston stand at the same place, on returning to the same temperature.

Before placing the piezometer in the hollow plug, a thin leather envelope, kept saturated with oil, is drawn upon it (with the intention of affording protection against the shock of firing), and when inserted the remaining space within the plug is filled with oil, which is retained by stopping the opening through the retaining-ring (which forms the communication with the bore of the gun), with a loosely-fitted disc of cork or leather.

One particular to be noted is the position of the eye of the piston with reference to the line in which the gun will recoil on firing. The metal surrounding the eye occupies a position at one side of the piston's axis of rotation in the barrel, thereby throwing the centre of gyration out of that line; and if that centre be so situated as to fall outside of a plane coincident with the line of recoil, it is evident that the piston will have a tendency to rotation when the gun is fired. It was apprehended that this rotation might interfere with the proper marking of the stem, or accurate measurement of the mark, and for that reason care was usually taken to place the piston in such position that its centre of gyration should be nearly in the plane just mentioned and forward of its axis of rotation.

After firing, the length of the stroke was measured under a compound microscope by the application of a scale divided into thousandths of an inch and capable of being read to ten-thousandths.

The volume of oil subjected to pressure, the area of the piston, and the length of stroke being known, we derive from them the degree of compression. To complete the data for ascertaining the pressure to which the piezometer has been exposed in any given case, it becomes necessary to determine the relation of compression to the pressure required to effect it.

The experiments made for this purpose were conducted with a good deal of care to secure accuracy, and were somewhat elaborate.

The barrel of the compressing pump is of cast-steel, placed horizontally, its exterior diameter being 2·2 ins., and that of its bore 0·7 ins. The piston, which is also of cast-steel, tempered, is forced in and retracted by means of a square-threaded steel screw 1·5 ins. in diameter.

These parts are placed in a strong frame of iron. The screw is turned by means of a ratchet-wheel, which forms its head, and a lever and pawl connected with it. To facilitate the retraction of the piston a crank is attached. Screw-valves prevent, during the alternate movements of the piston, the escape of the liquid through the aperture by which it enters the barrel, and its return from the receiver into which it is forced. A safety-valve of tempered steel, ground to a seat, also in hardened steel, with its graduated lever and weight, is attached to the pump, and serves for measuring the pressures it is made to exert.

A comparison of the pressure of a column of mercury of known height, with the weight on the valve of the gauge required to balance it, was adopted as the most accurate method of determining the relation between the weights to be placed on the scales and the desired pressures per square inch.

The mercurial pressure-gauge consists of a cistern of glass containing mercury, and a series of perpendicular glass tubes attached to a graduated staff and joined continuously together, extending to the height of fifty-two feet. The tubes are so connected with the cistern that by pressure upon the surface of the mercury it may be forced to ascend the tubes.

In computing the pressure of the column, its height from the surface of the mercury in the cistern was taken, and the counteracting pressure of the weight of the oil by which the pressure was applied was allowed for in proportion to its height and specific gravity.

To ascertain the compressibility of oil in steel piezometers, it was filled and set in the same manner as for use in the gun. The receiver was immersed in a water-bath kept at the proper temperature. The pressures, temperatures, and lengths of stroke are given below ;—

Compression of Oil in Steel Piezometer.

Pressure. Per square inch. Lbs.	Length of stroke at— 60° In.	50° In.
10,000	0·2148	0·2401
15,000	0·2780	0·3266
20,000	0·3656	0·4108
25,000	—	0·5670

We now arrive at the experiments in which the piezometer was used to record the compressions produced by the pressure of fired gunpowder under various conditions, and from which the pressures were to be derived. They were made at Washington Arsenal, D.C., commanded by Major Alfred Mordecai, who heartily co-operated in their conduct.

Two six-pounder guns, one of iron the other of bronze, were used in these experiments. The diameter of the bore of each, at the seat of the shot, was 3·69 ins., very nearly. The iron gun was used in the first three experiments, in which the piezometer was attached to the bottom of the bore. It was afterwards pierced through its side to receive the piezometer inclosed, as described in the Report, in a hollow steel plug, the centre of the opening being 1·5 ins. forward of the bottom of the bore. It was used in this form in several experiments. The bronze gun was, however, used in the greater number. It was pierced with nine holes at different distances from the bottom, beginning at 1 inch and ending with 47·8. They were arranged alternately to the right and left of the central vertical plane, in the upper half of the gun, and inclined 45° to that plane.

A solid plug was fitted to each hole, and was withdrawn only to permit the insertion of the piezometer.

Small holes to receive a thermometer with an elongated bulb were drilled near the openings, mentioned above, extending to within a short distance from the bore.

Several experiments were made to ascertain pressures in a musket barrel. A portion of the bore at the breech-end was enlarged enough to receive the piezometer, and was separated from the forward portion by a ring screwed to place and a leather disc closing its opening, as described in connection with the hollow plug. The piezometer was introduced from the rear, the surrounding space filled with oil, and the breech-plug inserted afterward. A vent of the normal size was drilled just forward of the partition formed by the ring and disc just mentioned.

It has already been intimated that the temperature at which the piezometer was set was that at which the gun was to be fired. In the earlier experiments the method by which it was intended to effect this was by inclosing the instrument, *ready* to be set, in the gun for a sufficient time to equalise their temperature—which was left to merely atmospheric influences. The instrument was then withdrawn, quickly set, and returned, and the gun fired without delay.

It was afterwards found that the gun changed its temperature sensibly, from changes of wind and sky, in shorter intervals than was supposed. To avoid the errors liable to arise from this cause the practice was changed. The instrument was set at a determinate temperature, higher than that of the outside air, but lower than that of the arsenal workshops. The gun was run into the shops (near by) for a short time in the interval between the experiments, and made slightly warmer than the firing temperature. The piezometer was then properly inserted, the gun taken outside, and when cooled to the proper point the thermometer was removed and the gun fired by a primer already inserted.

The average pressure indicated at different distances from the bottom of the bore, the charge being in each case 1½ lbs. of powder, in a cartridge-bag, and one 6-pounder shot, is given below:—

Distance from the bottom of bore.	Pressure per sq. in.
1-inch (bronze gun)	20,210 lbs.
1½ ,, (iron gun)...	18,150 ,,
4 ,, (bronze gun	16,510 ,,

The capability of the piezometer to measure pressures of much more brief duration than the time of the passage of the ball through the barrel will appear from a comparison of the inertia to be overcome in each case. The weight of the piston and stem is 20 grs., and the resistance of the inertia of the oil is equivalent to that of about 36 additional grs., while the weight of the pistol ball is 218 grs. After taking into account the differences of area and the mode of action in the two cases, I think it may be safely concluded that the piston will have reached its extreme position as soon at least as the ball has passed through the same distance.

The superior practical importance of determining the *greatest* instead of the *average* pressure upon the sides of the bore, with reference to the construction of guns, is evident from the fact that the strength of the gun must be adapted to the former and not to the latter. The inadequacy of the mode of determining, even relatively, the amount of pressures of very brief duration by the initial velocities of balls projected by them is shown by the effects of detonating powders used in fire-arms: the barrel may be shattered while the ball is thrown with comparatively little force.

The effect of increasing the charge of powder, compared with that of increasing the weight of the projectile, in augmenting the pressure was unexpected, and has to some seemed almost incredible; although, as is not surprising in regard to a subject of such a nature, opposite views are held by men thoroughly conversant with artillery.

	Lbs.
The mean of the pressures indicated at 1 inch from the bottom of the bore, with a charge of 1½ lbs. of powder and a ball weighing, with its sabot, 6·3 lbs. nearly, was	20,210
With the same charge, at 1½ ins. from the bottom	18,150
With 1½ lbs. powder and a shot weighing 12·15 lbs., at 1½ ins. from bottom...	20,743
With 2 lbs. powder and a ball of 6·36 lbs., at 1 in. from bottom	20,640
With 3 lbs. powder and a ball of 6·43 lbs., at 1 in. from bottom	22,220

The pressure in the cases in which shot of 12·15 lbs. weight were used, reduced to that which would be indicated at 1 inch from the bottom of the bore, on the supposition that the same difference would exist as with the smaller charges, would be 22,700 lbs., indicating that the effect of doubling the weight of the ball (but with diminished windage) is not very different from doubling the weight of the powder —instead of increasing the pressure in a much greater ratio, as some have supposed.

The results of the experiments with the musket barrel accord, so far as they admit of comparison, with those just stated. The manner in which an increased charge of powder may be supposed to affect the pressure has already been referred to. The resistance of the forming gases in that part of the charge which is least confined, to the expansion of those in other parts of the charge, is perhaps most strikingly illustrated by the action of the fulminates, which, in quantities of a few grains, will, as is well known, tear in pieces a brass or copper plate upon which they are gently heated.

The variations of pressure sustained by the gun when fired with charges very nearly the same are greater, as might be expected, than the variations of initial velocity imparted to the ball under similar circumstances.

When the combustion of the powder takes place with more than average rapidity, the pressure in the first instants of the explosion is augmented, but its action on the ball is not so well sustained as in the case in which the combustion is more slow and consequently longer continued.

In an article "On the Pressure of Fired Gunpowder in its Practical Applications," in the "American Journal of Science and Arts," September, 1856, Dr. Woodbridge, after reference to the experiments of Count Rumford, who estimated the pressure of gunpowder fired in a space which it filled at not less than 54,750 atmospheres—from an erroneous estimate of the strength of his eprouvette, which was burst by the charge—gives the following experiment, which seems to show that the extreme force of gunpowder fired in small quantities does not exceed 6,200 atmospheres:—"I inclosed in a hollow cylinder of cast-steel, 1⅛ ins. in exterior diameter and one-fourth of an inch in diameter interiorily, 20 grs. of Hazzard's Kentucky rifle powder, which filled loosely the cavity. This was fired by a flash of powder penetrating through the aperture of a valve (of steel) opening inward, but designed to prevent the escape of gas outward. The cylinder was not ruptured, and, being put under water, no gas was found to escape; (the weight of the instrument was too great to test the loss of gas by my scales.) On pressing in the valve by means of a screw, an abundance of gas escaped, carrying with it the odour of sulphuretted hydrogen. The seat of the valve was found to remain perfect; a fact which, when compared with a former trial in which the gases escaped in consequence of a slight defect of the valve, is presumptive proof of its immediate action. The residuum was found to weigh 10·45 grs. The calculated strength of the cylinder would be equal to an internal pressure of about 93,000 lbs. per square inch, or 6,200 atmospheres of 15 lbs." Dr. Woodbridge regards the method of ascertaining powder pressures by the registered compression of a liquid as capable of greater accuracy than those which are based upon indentation or other change of form in pieces of metal acted on by a piston receiving the pressure to be measured.

NOTICES OF BOOKS.

Rhymes of Science: Wise and Otherwise. New York: Industrial Publication Company.

We have here a collection of poems referring to Science chiefly from a sarcastic point of view. There is "Sir Thomas the Good," extracted from the "Ingoldsby Legends;" there is the well-known "Society upon the Stanislow," and various other effusions of a kindred stamp. There is not, however, Peter Pindar's account of the insect-hunting adventures of Sir Joseph Banks, which we should strongly recommend to the compilers if they feel disposed to produce a companion-volume. The weak side of such verses is that their point, and even their very intelligibility, depend on allusions which are soon forgotten. Thus we can understand that the "De Sauty" of Dr. O. W. Holmes treats of the first laying of the Atlantic telegraph-cable. But what "Cyano-Rhinal" or "Ceruleo-Nasal" can mean is a mystery. We should take them to be "pet names" for that most disreputable of Primates known as *Cynocephalus mormon.* Again, we have the translation of a song given at the concluding dinner of the convention of German physicians and naturalists, headed "Science on a Bender." The allusion here escapes us.

"Jim Green" is a rhyme of finance rather than of science.

Notes on Building Construction, arranged to meet the Requirements of the Syllabus of the Science and Art Department of the Committtee of Council on Education, South Kensington. Part III. Materials.—Advanced Course and Course for Honours. London, Oxford, and Cambridge: Rivingtons.

We have here a wonderful collection of information,—geological, chemical, physical, and even to some extent botanical and zoological,—together with much lore not falling within the boundary-lines of any of the sciences, the whole having a bond of unity in the fact of its being necessary for builders and architects. The author, whoever he may be, and the publishers must be congratulated, at least as far as we are able to judge, on the general accuracy of the work. The only questionable statement we have found is in the chapter on Pigments, where Persian red is stated to be a "chromate of lead, produced by boiling white lead with a solution of bichromate of potash." The colour we have met with under this name is a compound of stannic acid and chrome, much more stable than any lead chromate, and approaching a madder-lake in its shade. Trade names, however, are used with such latitude that we should hesitate to pronounce the passage quoted to be erroneous.

A peculiarity of this book, if we consider the especial purpose for which it is written, is the retention of the "old" chemical nomenclature, for the very satisfactory reason that it is more generally intelligible. We should have thought, however, that South Kensington would have been prepared to fulminate the greater excommunication against the luckless wight who in these days should speak of carbonic acid as a constituent of the atmosphere.

We should consider the work an admirable manual of reference for builders, but the demands made upon the verbal memory of the unfortunates who have to be examined in it are simply fearful to contemplate.

An Introduction to the Systematic Zoology and Morphology of Vertebrate Animals. By A. Macalister, M.D. Dublin: Hodges, Foster, and Figgis. London: Longmans and Co.

Borrowing Earl Beaconsfield's critique on the poetry of Mr. R. Browning, we might couple with our appreciation of the merits of this work the wish that some good soul would translate it into English. Let us suppose it placed in the hands of a man well versed in the science of zoology as taught in the middle of the present century, but who had spent the last twenty years on some desert island. He would naturally expect to find that many new discoveries had been made, and that many notions accepted in his day had been given up. But to what extent such changes in the matter had taken place he would be unable to detect, on account of the still greater and more obtrusive revolution in the manner. He would find himself hopelessly lost in a fire-new terminology. Biology has not, indeed, like chemistry, given names of ten and twelve syllables in length to the individuals she describes. No one, fortunately, has yet conceived the idea of making the generic and specific names of an animal or plant embody its supposed line of descent. All this will come by-and-bye unless a vigorous stand be made. But for groups and classes of higher rank than genera, as well as for the parts of animals and for their structural attributes, the names selected are needlessly long and gratuitously un-English. We always presume that in every branch of natural science the great object of the teacher should be to concentrate the attention of the student upon the facts, the phenomena described, and for that very reason to burden his memory as little as possible with words. Too many professors seem to be of the very opposite opinion. The slightest excuse is enough to justify the introduction of a new term. This tendency is particularly to be regretted in the present day. The division of labour, which has rendered such invaluable services in the industrial arts, assuredly must and will extend to the realms of science and learning. We have already numbers of eager students in biology who know "little Latin and less Greek," and their number will assuredly increase. Why, then, should treatises be written which such men cannot understand—if at all—without great loss of time? Prof. Macalister—who is neither the only, nor perhaps the greatest, sinner—seems conscious that his terminology is open to criticism. He remarks, in his Preface, that it is "impossible to write a strictly scientific treatise in popular language without increasing its bulk proportionally." Had we time we think we could undertake to render his book, not indeed into "popular language," but still into language such as a mere English scholar, guiltless of Greek, would clearly understand, and that without increase of bulk. Take, for instance, two ugly neologisms, artiodactyl and perissodactyl—why not say simply even-toed and odd-toed? Here, by using plain English, we economise space without sacrificing intelligibility.

Birds of the Colorado Valley. A Repository of Scientific and Popular Information concerning North American Ornithology. By ELLIOTT COUES. Part I.—Passeres to Laniidæ. Washington: Government Printing-Office.

WE have here the first part of a most valuable work—one of the many collateral publications issued in connection with that wonderful undertaking, the United States Geological and Geographical Survey of the Territories. The author describes fully the structural characteristics, the habits, the geographical distribution, and the synonymy of every bird-species known to occur in the Colorado Valley. The region in question corresponds approximately with the "middle faunal province" of some authorities. It is bounded on the east by the main water-shed of the Continent, the Rocky Mountains, and of the Sierra Nevada on the east. Its northern limit is the Salt Lake Valley, and to the south it fades gradually into the great Neotropical region, many of its characteristic forms being still traceable on the table-lands of Mexico. The district offers wonderful variations of climate and surface, and altitude—as the author remarks—does the work of latitude. The characteristics of the work are accuracy and thoroughness; its only defect, in our opinion, is a disposition to indulge in irrelevancies, as may be seen in the essay on the Cat-bird, very pleasant reading though it be.

The chapter on Swallows is admirable. The question whether certain individuals of the various species of this group, and of the swifts, do not occasionally hybernate, is discussed at great length; and the author, whilst fully admitting that the vast majority of these birds migrate, does not feel able to refute the evidence in favour of a minority passing the winter in northern climates in a state of torpor. The literature of this subject, chronologically arranged, extends to more than twelve pages, but curiously enough no mention is here made of Gilbert White, who repeatedly refers to the subject in his "Selborne."

The architecture of the swallows is also considered at some length. The interesting fact is brought into due prominence that, with one possible exception, all the species have modified the structure of their nests in accordance with the novel facilities afforded by the settlement of the country. The case of the common European house martin is still more instructive, since within the last fifty years it has changed the shape of its dwellings, not in accordance with any modification in the character of the site selected, but simply with a view to increased convenience.

Dr. Coues considers that the original and typical colour of the eggs of swallows, like those of hole-diggers in general, was a pure white, but that they have gradually become speckled as the nesting-habits of the bird have undergone modification.

Swallow-shooting the author denounces with just severity, and he sympathises warmly with these amiable and useful birds in the defensive warfare they are now obliged to wage against those "wretched interlopers" the European sparrows, whom some wrong-headed acclimatiser has, it appears, rashly introduced into America. We hear with regret that the same blunder has been repeated in Australia. Whilst upon the subject of acclimatising it may not be deemed an unpardonable digression if we record our disgust at learning that an Expedition, which ere this has doubtless sailed from Australia to New Guinea, is about to introduce goats into that country—for the purpose, we presume, of destroying its magnificent flora.

Space will not permit us to extend any further our notice of this work, which we must pronounce a sterling contribution to zoological science.

Proceedings of the Literary and Philosophical Society of Liverpool. Sixty-seventh Session, 1877-78. No. XXXII. London: Longmans and Co. Liverpool: D. Marples and Co. (Limited).

We have on former occasions noticed with regret that in the "Proceedings of the Liverpool Literary and Philosophical Society" science plays a part somewhat similar to that of the "one halfpenny worth of bread" in Falstaff's tavern bill. It might, indeed, have been hoped that under the successive presidencies of the Rev. H. H. Higgins and Dr. Drysdale, both of whom have honourably won their spurs in biological research, a better day might have dawned. Such, however, is not the case. A little work has, indeed, been done, and is noticed in the reports of the successive meetings. Thus Mr. T. Higgin, F.L.S., communicates an interesting notice of a fresh-water sponge (*Spongilla coralloides*) from the rapids of the River Uruguay. Mr. E. D. Jones, a corresponding member, residing at Sao Paulo, in Brazil, is making good progress in recording the metamorphoses of the Lepidoptera of the district: he has observed a caterpillar, belonging to the Bombycidæ, which, if annoyed, utters a low but distinct musical sound. Of fifty species of caterpillars which he has examined, twenty at least possess venomous spines: one of these, a very hairy species, of a bright orange, when applied to the back of the hand produced a most intense pain, which lasted for more than twelve hours, and extended to the arm-pit.

Mr. F. P. Marrat briefly describes a collection of shells from Fuca Straits and Cape Flattery, presented to the Liverpool Museum by Dr. D. Walker.

Mr. T. Higgin communicates notes on the polypidom of the Hydractinidæ, which attach themselves to the calcareous shell of a mollusk, and by some unknown process convert it into a skeleton for themselves.

Mr. J. Roberts describes the geological results of the borings at East Hoyle Bank, and the Rev. H. H. Higgins enters into some speculation on the peculiar structure of the jaws of *Angosoma Neptunus*.

When we turn, however, to the *pieces de resistances*, the papers selected for publication in full and forming the body of the volume, we find not even one composed of the results of experimental research or original observation.

The Presidential Address on the question "Is Scientific Materialism compatible with Dogmatic Theology?" has already come under our notice.*

---

* See Quarterly Journal of Science, vol. viii. (n.s.) p. 133.

A paper—or shall we say a sermon—in reply, by Mr. J. A. Picton, entitled "On Scientific Materialism from a Non-scientific Point of View" contains a very remarkable utterance:—"If there is one point more than another insisted on by Evolutionists it is the denial of the possibility of miracles." Would Mr. Picton be surprised to learn that the ablest reply extant to Hume's attempted demonstration of the impossibility of miracles is from the pen of one of our most eminent English Evolutionists? Perhaps, however, the word "Evolutionist" is used in some special—perhaps local—acceptation. This is the more probable because in a paper by the Rev. H. H. Higgins, entitled "Developmentalists and Evolutionists, or the Use of Dogma in Science," we find the term "Evolutionist" used in a sense which we never met with before, and which we cannot for one moment accept. It has been used as the common generic name, including alike Messrs. Lamarck, Darwin, Wallace, Haeckel, Asa Grey, St. George Mivart, J. J. Murphy, Oscar Schmidt, Leconte, J. J. Allen, Huxley, Spencer, Belt, Bates, and their followers. If this term is taken away and applied to some particular though not very clearly characterised section who are to be distinguished from certain "developmentalists," we have no common name left for all those naturalists who recognise development as opposed to individual creation.

The essays on the Credibility of Venerable Bede, on Amy Robsart, on the Proverbs of European Nations, on Moses Mendelssohn, and on Trevelyan's Macaulay, do not come within our cognizance.

To one, indeed, of these literary papers we feel compelled to refer. Mr. E. R. Russell, in his essay on Trevelyan's Life of Macaulay, gives utterance to these sentiments:—"He never thought it worth while to quit more attractive studies for the blind and groping physicism which now almost monopolises the name of Science. Whatever good it may have done in other directions, physical science has of late discouraged and debilitated moral and historical inquiry, which is of much more value to the world!" It has always seemed to us, as well as to far abler men, that in England at least—on account of the preponderant attention paid to history, to scholarship, oratory, and polite literature—physical science is neglected, and that by reason of this very neglect we are drifting more and more into the background as compared with some of our neighbours.

The passage we have quoted shows how little sympathy and compatibility exist between literature and physical science, and how injudicious, to say the least, is the attempt to form societies for their joint cultivation. Whereas in England public interest centres in words rather than in things, in criticism and commentation rather than in observation and experiment, literature will always succeed in engrossing more than the lion's share of time and resources, as the volume before us but too plainly proves.

Practical Hints on the Selection and Use of the Microscope. Intended for Beginners. By John Phin, Editor of the "American Journal of Microscopy." Second Edition. New York. 1877.

This little book will prove of great use to those for whom it is intended. The construction of the microscope is treated in a simple manner, so as not to confuse the beginner. The hints as to the selection of an instrument are very good, and especially adapted to a country like America, inundated with foreign microscopes, good, bad, and indifferent. These remarks will help the student to avoid purchasing what is useless to him.

The portion devoted to manipulative processes is well compiled, the directions plain and easy to follow.

The Chemistry of Common Life. By the late James F. W. Johnston, F.R.S., &c. A New Edition, revised and brought down to the present date, by Arthur H. Church, M.A., &c. London and Edinburgh: W. Blackwood and Sons. 1879.

The late Prof. Johnston had a peculiar talent which is possessed by very few scientific writers. In common with Faraday, Liebig, Tyndall, Huxley, and a few others, he enjoyed the power of being able to clothe the dry bones of scientific fact with the warm and living flesh of lucid exposition and apt illustration.

It is now nearly twenty years since the last edition of Prof. Johnston's work was published, under the editorship of the late Mr. G. H. Lewes. Prof. Church, however, has had a great advantage over his predecessor, inasmuch as he had the opportunity of consulting Prof. Johnston's private and corrected copy of the "Chemistry of Common Life." Little has been added to Prof. Johnston's original work, except in the way of correcting certain figures and statements. Prof. Church has performed his task of revision with great judgment. He tells us in the Preface that he has been compelled to add one entirely new chapter, which no doubt was suggested by the perusal of Prof. Johnston's own notes. It is entitled "The Colours we Admire." In this chapter Prof. Church tells how brilliant colours of all hues have been got out of the coal-scuttle; how chemists have entered into competition even with the great Cybele herself, and have produced alizarin in the test-tube with as great facility as the faithful goddess herself has done in the madder-root; how copper exists in turacin, the colouring-matter of the beautiful crimson wing-feathers of the touraco, a West African bird, and iron in the hæmoglobin of the blood of mammals. We also have a description of chlorophyll and colein, the latter a colouring-matter discovered by Prof. Church in the leaves of the *Coleus Verschaffeltin.* This chapter might have been extended with great advantage. A copious index adds much to the value of the work.

CORRESPONDENCE.

MENTAL INANITION.

To the Editor of the Monthly Journal of Science.

Sir,—May I ask whether sufficient attention has been paid to the following curious analogy? It is well known that external agencies, such as malaria, make the most prompt and deep impression upon the human body when in a state of inanition. In like manner, objects seen, events witnessed, or words overheard when we are in a state of mental inanition, stamp themselves upon the memory with a permanence out of all proportion to their interest or importance. Thus, having once to walk for about 10 miles through a most uninviting country, on a very dull day, in order to escape the greater evil of waiting for two hours at a railway-station, I saw a butcher washing out an inkstand in the street just outside his shop. It is difficult to conceive a more insignificant occurrence, and yet it has remained as if burnt into my memory. I could give other matters equally trifling, remembered because at the time of their happening my mind was, so to speak, hungry for impressions.—I am, &c.,

S.

THE SEA-SERPENT.

To the Editor of the Monthly Journal of Science.

Sir,—Referring to "Sceptic's" question respecting the Sea-Serpent, in your April number, there is no doubt that Captain Drevar and his mate, of the barque *Pauline*, deposed to the truth of their story, of having seen a sea-serpent, before a magistrate at Liverpool. Capt. Drevar's story is related by the Rev. E. L. Penny, Chaplain to H.M.S. *London*, at Zanzibar, in the "Illustrated London News" for November 20, 1875.—I am, &c,,

M. H. Close.

40, Lower Baggot Street, Dublin,
April 23, 1879.

PROCEEDINGS OF SOCIETIES.

Royal Society, *April* 3.—"On the Thermal Conductivity of Water," by J. T. Bottomley. The experiments described in this paper were conducted by a method devised by Sir William Thomson. The liquid whose thermal conductivity was to be determined was heated from above, to avoid convection currents. Two methods of heating have been used. In one, a horizontal steam chamber was applied at the top of the water or other liquid, and, steam being continuously passed through the heating chamber, the surface of the liquid under experiment was kept at a very high temperature, and heat was conducted from above downwards. In the other method a large quantity of very hot water was deposited on the top of a mass of cold water, mixing being prevented by a simple contrivance, and the heat of this superincumbent layer was conducted downwards through the colder water below. The experiments were carried on in very large vessels, or tanks, in order to avoid disturbance by means of loss of heat at the sides. Three principal thermometers were employed; together with a fourth, whose object was merely to show when heat begins to be lost at the bottom of the layer of fluid experimented on. When this loss commences the experiment is at an end. The other three thermometers were used thus:—First there was a thermometer with a bulb 30 centims. long, which was placed vertically, its object being to show the average temperature from top to bottom of the layer of fluid bounded by horizontal planes passing through the top and bottom of its bulb. The rise of this thermometer in any time shows the quantity of heat that has passed into the stratum occupied by it in that time. The other two thermometers were placed with their bulbs horizontal, and one at a known distance vertically above the other. They indicate the temperatures of the layers in which they are placed. Knowing the difference of temperatures of two sides of a stratum of a liquid during any time, and the quantity of heat conducted across the stratum during that interval of time, the thermal conductivity of the liquid can be calculated. The result arrived at by the experiments described is that the thermal conductivity of water may be taken at from 0·0022 to 0·00245 in square centimetres per second. The author has made some experiments on the thermal conductivity of solution of sulphate of zinc. These experiments are now being carried on with the assistance of a grant from the Government Fund of £4000.

"Note on the unknown Chromospheric Substance of Young," by G. D. Liveing, M.A., Professor of Chemistry, and J. Dewar, Jacksonian Professor, University of Cambridge. In the preliminary catalogue of the bright lines in the spectrum of the chromosphere published by Young in 1861, he calls special attention to the lines numbered 1 and 82 in the catalogue, remarking that "they are very persistently present, though faint, and can be distinctly seen in the spectroscope to belong to the chromosphere, as such, not being due, like most of the other lines, to the exceptional elevation of matter to heights where it does not properly belong. It would seem very probable that both these lines are due to the same substance which causes the D_3 line." Again, in a letter to "Nature," June, 1872, Young says, "I confess I am sorry that the spectrum of iron shows a bright line coincident with 1474 (K); for, all things considered, I cannot think that iron vapour has anything to do with this line in the spectrum of the corona, and the coincidence has only served to mislead. But there are in the spectrum many cases of lines belonging to the spectra of different metals coinciding, if not absolutely, yet so closely that no existing spectroscope can separate them; and I am disposed to believe that the close coincidence is not accidental, but probably points to some physical relationship, some similarity of molecular constitution perhaps, between the metals concerned. . . . So, in the case of the green coronal matter, is it not likely that though not iron it may turn out to bear some important relation to that metal?" In 1876 he proves that the coronal line 1474 is not actually coincident with the line of iron. In the catalogue of bright lines observed by Young at Sherman, in the Rocky Mountains, it appears that the above-mentioned lines 1 and 82, along with D_3, were as persistently present as hydrogen, the only other line approaching them in frequency of occurrence being the green coronal line 1474 of Kirchhoff, which was present on ninety occasions out of a hundred. It occurred to the authors that these four lines may belong to the same substance. An analogy in the ratio of the wave-lengths of certain groups of lines occurring in different metals has been already pointed out by Stoney, Mascart, Salet, Boisbaudran, and Cornu; and the near coincidence observed by the authors in the ratios of certain lines of hydrogen, lithium, and magnesium, substances belonging to the same type, combined with a similar ratio in the wave-lengths of the nearly equally persistent lines of the chromosphere, greatly strengthens the probability of the assumption that these lines belong to one substance. The fact that the two less refrangible rays have no representative in the Frauenhofer lines is by no means opposed to their belonging to one substance, since aluminium behaves in a similar way in the atmosphere of the sun; and in the total eclipse of 1875 the hydrogen line *h* was not visible in the chromosphere, but was on the limit between brightness and reversal; and during the late eclipse the two most refrangible rays of hydrogen were not detected from the same cause.

CHEMICAL SOCIETY, *April* 3.—Mr. G. Attwood read a paper "On a Gold Nugget from South America." In the State of Guayana, Venezuela, a large area of alluvial soil has lately been found to contain gold, and nuggets up to 25 ounces have been discovered within 3 feet of the surface. Numerous gold-bearing quartz veins are found in the neighbouring hills. Quite one-half of these nuggets are covered with a dark brown substance, resembling a silicate of iron. Such a nugget was treated with hydrochloric acid (its weight diminished, after treatment with HCl and NaO, from 304·7 grains to 284·33 grains). The solution contained—Silica, 0·12 gr.; ferric oxide, 8·88; lime, 0·15; magnesia, 0·08. The nugget was then treated with caustic soda, and again with HCl. The solution contained—Silica, 4·66 grs.; ferric oxide, 4 60; lime, 0·21. During this process much gold in a finely divided state became detached, and after the treatment the nugget was partly covered with a coating of finely divided gold, of a dull colour. The nugget contained 94·54 per cent of gold. The gold from the quartz veins contains 87·9 per cent gold. From these experiments the author concludes that gold nuggets gradually increase in size, owing to the accumulation of fresh particles of finely precipitated gold. Specimens of these nuggets showing the dark glazed coating were exhibited, including one weighing over 14 ounces.

INSTITUTION OF CIVIL ENGINEERS, *March* 25.—Mr. Bateman, President, in the chair. The paper read was on "The Electric Light applied to Lighthouse Illumination," by Mr. J. N. Douglass, M. Inst. C.E. The author showed the progress of lighthouse luminaries from wood and coal fires to the introduction of tallow candles, fatty oils, mineral oils, coal gas, and electricity. In 1839 experiments were made by Faraday, for the Trinity House, at the Orford Low Lighthouse, with the Bude light, and, in 1862, at the South Foreland with the Drummond or limelight, but the results were not so satisfactory as to lead to their adoption. In 1857 experiments were tried at Blackwall with the electric light, produced by the first magneto-electric machine of Holmes; and on the 8th of December, 1858, the electric light, obtained by Holmes's second machine and a "Duboscq" lamp, was shown on the sea for the first time from the South Foreland High Lighthouse. On the 1st of February, 1862, the Trinity House exhibited the electric light permanently at Dungeness Lighthouse, by Holmes magneto-electric machines and lamps. The works for the production of the light were described, and the first cost and maintenance given. The intensity of the electric luminary was about 12½ times that of the oil luminary. The cost per unit of light per hour was 0·1165d. for the oil, and 0·1294d. for the electric light. Frequent falling off of the latter light occurred, and the oil light had occasionally to be substituted. Shortly afterwards the French lighthouse authorities established the electric light at Cape La Hêve, with the magneto-electric machines of the Alliance Company of Paris. In 1867 Holmes further improved his machine and lamp. Two of these machines and lamps were exhibited by the Trinity House at the Paris Exhibition of 1867, with a dioptric apparatus of the third order. The Souter Point Lighthouse was lighted by electricity in January, 1871. The light was adapted to a dioptric apparatus of the third order. A lower light from the same luminary as the upper one was adopted here for the first time. The optical apparatus for both lights was designed by Mr. James T. Chance, M.A., Assoc. Inst. C.E. A "Holmes" fog trumpet apparatus was also worked from the same engines as the electric light apparatus. The total cost of the works was £18,000. The cost of the electric luminary per candle per hour was 0·056d., being rather less than half of the cost per unit at Dungeness. The maximum intensity of the beam from this apparatus was about 700,000 candles. The Trinity House next established the electric light at the South Foreland High and Low Lighthouses, in January, 1872. The apparatus for the production of the light consisted of two steam engines of 20 effective horse-power, four Holmes's improved magneto-electric machines and lamps, and two dioptric apparatuses of the third order for fixed white light. The cost of the additional works for these lights was £14,800. The intensity of the full power beam from the High Lighthouse was about 20 times that of the old first order dioptric oil light. The relative cost per unit of light was as 100 oil to 30·6 electric. In 1873 the Trinity House adopted, for the Lizard Lighthouses, Siemens's dynamo-electric machine and lamp, and a siren fog signal. These were driven by three of Brown's caloric engines, each of 10 effective horse-power. The cost of the additional works for these lights was £14,936, and the annual maintenance, including interest on first cost, amounted to £2,365 6s. 4d. against £1,016 7s. 11d. for the oil lights. The intensity of the full power beam of each light was about 330,000 candles, being about 211½ times the intensity of the oil light. The relative cost per unit of light was 100 oil to 14·04 electric. The successive improvements in the electric machines, and in the means of driving them, had reduced the cost of the electric light at the Lizard to one-ninth of that at Dungeness, and the quantity of light produced at the Lizard per pound of coke consumed was increased 20 times. The continued growth seaward of the shingle point at Dungeness led to the removal, in 1876, of the original electric light apparatus, and the substitution of a low flashing oil light and siren fog signal for both lighthouses. The author furnish-d information received from M. Allard, Director-General of the French Lighthouses, relative to the electric lighthouses at Cape La Hêve and Cape Grisnez, where Alliance magneto-electric machines and Serrin lamps had been adopted. It was intended to exhibit the electric light in the present year in a new lighthouse on the Isle of Planier, opposite Marseilles, and it had been decided to substitute the electric light for the oil light in the Palmyre Lighthouse at the mouth of the Gironde. Some information was also given relative to the electric lighthouses at Odessa and Port Said, these making ten in which the electric light had already been established. The comparative cost and efficiency were shown of lighthouse luminaries produced by all the agents at present employed, viz., oil, coal gas, and electricity. Coal gas, on Wigham's system, was applied to the Howth Bailey Lighthouse, Dublin Bay, by the Commissioners of Irish Lights, in June, 1865, and it had since been extended to seven lighthouses on the coast of Ireland. In 1872 the Trinity House adopted it at the Haisborough High Lighthouse. The additional cost of the works necessary for the introduction of gas at this station was £1,996, and the annual maintenance of the gas establishment, including interest on first cost, &c., amounted to £832 4s. 3d. This light had a mean intensity of 1,173 candles, and a maximum intensity for thick weather of about 2,923 candles. In 1877 the necessary additions were made for lighting the Low Lighthouse (distant 776 yards), by gas from the same works, at a cost of £1,296. The system of Mr. Wigham had been further developed by introducing the flames of two, three, and four large burners over each other in the axis of the dioptric apparatus. In January of last year the Commissioners of Irish Lights adopted one of the latter lights in a new lighthouse at Galley Head, near Kinsale. The maximum intensity of the four burners combined for thick weather was about 5,012 candles. The author next gave statements showing the comparative focussing compactness of the lighthouse luminaries which had been referred to for utilising in optical apparatus, viz., the lights produced by oil, coal gas, and electricity. The focussing superiority of the electric luminary, compared with the best of these, was as 616 to 1. Statements were given of the comparative average cost and annual maintenance of a single lighthouse (shore station) in this country, with colza oil, mineral oil, coal gas, and electricity, as the illuminating agents, both with and without a first-class 20 horse-power siren fog signal. For a maximum degree of light equal to the single or combined intensity of the luminaries of the Lizard, the cost of the more perfect electric luminary per unit of light provided was about 13-22 and 6-22 respectively of that of coal gas, and about 13·65 and 6·65 respectively of that of mineral oil, at their maximum intensities. With higher intensities of the electric luminary the cost per unit would be more in its favour, no further addition to the working staff being necessary. From experiments by Faraday for the Trinity House, in 1836, relative to the penetrative power of lights, through such obstructions as fog, mist, &c., and the more recent experiments by the French Lighthouse authorities and by the Trinity House, with oil and electric lights, it might be assumed that, with the atmosphere so impaired for the transmission of light, that the oil luminary at its maximum intensity would be fairly visible at the fog signal range of two miles, the electric luminary at its double Lizard intensity of 16,500 candles would be visible at about four miles. Further, that on more frequent occasions, when the oil luminary would be visible at about eight miles and a-half, the electric light would be visible at the full range of 17 miles.

April 22.—Mr. Bateman, President, in the chair.

The paper read was on "Dioptric Apparatus in Lighthouses for the Electric Light," by Mr. James T. Chance, Assoc. Inst. C.E. The author briefly premised that in the Fresnel or dioptric system the source of light occupied the central position within a structure of glass zones, or annular segments, by which the incident rays were condensed and directed on the sea; and that there were two principal kinds of dioptric apparatus, the fixed and the revolving. He then proceeded to make some observations concerning the different optical treatment which a small radiant like the electric arc required from that which suited an ordinary flame; in the latter case, as—for it concerned sea-lights—the object was not only to parallelise all the rays emanating from any point of the luminary, but also to reduce the vertical divergence due to the height of the flame by increasing the diameter of the optical instrument. On the other hand, the smallness of the electric arc afforded the opportunity of obtaining from the dioptric zones or other elements, by suitable generating sections, whatever divergence, whether horizontal or vertical, might be desired. It was also pointed out that the source of light, in the case of the electric arc, could not be entirely depended upon for maintaining the same position in relation to the focal horizontal plane, and that consequently—since the vertical divergence due to the luminary would move upwards or downwards with any vertical displacement of the radiant itself—the mariner could not be absolutely secured from failing to see the light, unless a special vertical divergence were given by the dioptric apparatus, independently of that caused by the size of the electric arc. This, however, involved the adoption for this illuminant of a dioptric instrument considerably larger than what was originally contemplated, so as to reduce materially the luminary divergence, and thereby be free to substitute for it, to some extent, a special vertical divergence. The author stated that in 1862 he had expressed himself in favour of a much larger apparatus than was then employed with the electric light at Dungeness. Also that, in 1865, Messrs. D. and T. Stevenson had recommended a third order apparatus for the purpose, in their Report to the Commissioners of Northern Lighthouses. A similar result was arrived at by the Elder Brethren of the Trinity House, in 1869, in consequence of comparative trials instituted by Prof. Tyndall, for testing the relative merits of a sixth order light, and a third order one respectively, when used with the electric radiant. The Souter Point revolving light, which was first exhibited in January, 1871, was described. Reasons were assigned for adopting two optical agents—one to condense the light in the vertical plane, the other to produce the required horizontal compression—instead of attempting, even for the refracting part of the apparatus, to effect the two condensations respectively by a single agent. Reference was made to the proposal of Mr. Thomas Stephenson, M. Inst. C.E., for attaining this latter desideratum; as likewise to that of Mr. Brebner, M. Inst. C.E., with a similar object. The method actually adopted was similar to that which had been already employed in France for the revolving light with the electric arc. It consisted of a fixed third order light encircling 180°, and of a rotating octagonal drum of the same height surrounding it. Each side of this drum, comprising three panels in height, was composed of vertical refracting prisms, by which the light, radiating in azimuth from the inner fixed apparatus, was compressed horizontally into a beam of 7° 8′ divergence in addition to that due to the diameter of the electric arc. This was done in such a manner that every single prism had its own independent divergence of the same amplitude, whereby was obtained an extent of light-emitting surface of a height of 6½ feet and of 22½ inches in breadth. Stress was laid upon its being the characteristic feature of the beam issuing from any one of the sides of this glass drum, that, in passing before the eye of the observer at sea, its brilliancy would, from first to last, remain unchanged, as distinguished from the waxing and waning appearance of the ordinary revolving light; consequently, at whatever distance the flash might be visible, the interval of its duration would be the same. Attention was also directed to the valuable suggestion made by Mr. J. N. Douglass, M. Inst. C.E., the Engineer to the Trinity House, that advantage should be taken of the landward hemisphere of the radiant light of the electric arc, to provide a beam which should be made to issue through a window in the tower below the main light, in order to mark certain dangers in Sunderland Bay; and it was stated that 54·6 per cent of the rearward hemisphere of light had been thus utilised. The two fixed lights which were inaugurated at the South Foreland, in January, 1872, were described. It was explained how the whole of the catadioptric zones, both upper and lower, were in both lights made to parallelise the rays in the usual manner. The light, however, incident on the refracting portion of each light was distributed over the sea from the horizon to within a short distance from each tower, by a succession of increasing angles of vertical divergence, so that the illumination of the sea became gradually diminished as the distance from land was lessened. In each light there was a rearward arc to spare, and this was turned to valuable account, from 67 to 71 per cent of this light being collected and acted upon by optical agents, which were particularly described, and thereby distributed uniformly over the front azimuthal arc, so as to intensify not only the illumination of the horizon and the distant sea, but also that of the nearer sea. It was mentioned that the two Lizard Lights, which were both fixed, and were first exhibited in March, 1878, had optical arrangements similar in every respect to those adopted at the South Foreland Lighthouses, with a slight variation in the refracting portions, arising from the circumstance that existing apparatus had to be turned to account in the construction of each apparatus. A table was appended, showing the condensing powers in the direction of the horizon of the lights described in the paper, distinguishing those optical portions which parallelised the incident light from those which gave to it special vertical divergence. According to this table, upon the assumption that the diameter of the electric arc was 12 millimetres, the condensing powers in the sea-horizon direction were as follow:—

Souter Point—Revolving	236·38
South Foreland—High fixed	50·17
South Foreland—Low fixed	43·40
Two Lizard Lights—Fixed	58·44

and data were added for adapting this table to particular cases. In a second table was given the respective condensing powers over the near sea, at different distances from the lighthouse towers.

METEOROLOGICAL SOCIETY, *February* 19.—Mr. C. Greaves, President, in the chair.

Among the papers read were the "Diurnal Variations of Barometric Pressure in the British Isles," by Frederick Chambers. The object of this paper is to show that differences of types of the diurnal variations of pressure at inland or sea-coast stations are due to the superposition, on a common type of diurnal variation at all the stations, of a distinct diurnal variation of barometric pressure, such as is required to satisfy the convection-current theory which explains the well-known diurnal land and sea breezes. To show this, all that is necessary is to take the differences of the corresponding hourly inequalities of the barometric pressure at pairs of inland and coast stations, and to exhibit these differences in the form of curves, which are then found to closely resemble the curves of diurnal variation of air temperature.

"On the Relation existing between the Duration of Sunshine, the amount of Solar Radiation, and the Temperature indicated by the Black Bulb Thermometer *in vacuo*," by G. M. Whipple, B.Sc., F.R.A.S. The author has instituted a comparison between the duration of sunshine, as determined by Campbell's sunshine recorder, and the amount of solar radiation, as ascertained from the readings of the black bulb thermometer *in vacuo*, for the year 1877, at the Kew Observatory. It is evident that there is a close relation between these phenomena, but, owing to the great range of the black bulb thermometer, the exact nature of the connection is not immediately evident. The author says that it may be safely concluded that the measure of solar radiation, as given by the black bulb thermometer, is only to be considered at any place as an indication of the relative presence or absence of cloud from the sky at the locality, and so its use as a meteorological instrument may with advantage be set aside in favour of the sunshine record, which has not the elements of uncertainty attached to it, inseparable from the former instrument.

March 19.—Mr. C. Greaves, President, in the chair.

The papers read included one on "Dew, Mist, and Fog," by George Dines, F.M.S. The author has, during the last two years, made a number of experiments to determine the amount of dew that is deposited on the surface of the earth. The plan adopted was as follows :—Glasses similar to ordinary watch-glasses were procured ; the surface area and the weight of each was ascertained. These glasses were exposed to the open air in the evening, being placed on different substances, viz., on grass, on slate, and on a deal board, the two latter being raised a few inches above the grass. A minimum thermometer was generally placed by the side of each glass. It is only on rare occasions that an amount of dew exceeding 0·010 inch in depth has been deposited upon the measuring glasses, and out of 198 observations in only three has that amount been exceeded : 58 observations give the amount from 0·010 to 0·005 inch, 107 from 0·005 to 0·001 inch, 22 less than 0·001 inch, and 8 observations no dew at all. The author thinks it may be fairly assumed that the average annual deposit of dew upon the surface of the earth falls short of 1·5 inch. There are two kinds of mist, the morning and evening. The morning mist is caused by the evaporation from the water and the moist ground taking place faster than the vapour is taken away ; the air becomes saturated, but this does not stop the evaporation ; the vapour continues to rise into the air, is there condensed, and forms mist, which gradually spreads over a wider surface. The evening mist is produced as follows :—The cold on the grass, caused by radiation, lowers the temperature of the air above it ; the invisible vapour of water previously existing in the air is in excess of that which the air can retain when the temperature is lowered ; the surplus is condensed, becomes a mist cloud, and floats in the air just above the surface of the grass. Taken either separately or combined, the mists appear to the author totally and altogether inadequate to account for those dense fogs which at times overspread large tracts of country. Dense fogs near the earth are often accompanied by a clear sky above, when the sun may be seen reflected from the gilded vanes of our public buildings. After long consideration the author is inclined to attribute these fogs to some cause at present unknown to us, by which the whole body of the air to some distance above the surface of the earth is cooled down, and as a consequence part of the vapour of that air is condensed, and forms what has been called an "earth-cloud."

"On the Inclination of the Axes of Cyclones," by the Rev. W. Clement Ley, M.A., F.M.S. In this paper the author calls attention to the evidence recently afforded by the results of mountain observations to the theory that "the axis of a cyclone inclines backwards." The author first reviews the state of the question up to the present time, and details his own investigations, chiefly founded upon the movement of cirrus clouds; he then refers to Prof. Loomis's recent "Contributions to Meteorology," in which is discussed the observations at the summits and bases of several high mountains, the results of which fully confirm the theory that the axis of a cyclone inclines backwards.

ROYAL INSTITUTION OF GREAT BRITAIN, *February* 21.—"A New Chemical Industry, established by M. Camille Vincent," by Prof. Roscoe, LL.D., F.R.S. Dr. Roscoe, after referring to the experiment by which ammonia was first obtained by Joseph Priestley in the gaseous state, and to the various ammoniacal discoveries which have been made during the last hundred years, remarked that in the French beet-root sugar industry, as in the manufacture of cane sugar, large quantities of molasses or treacle remain behind after the whole of the crystallisable sugar has been withdrawn. These molasses are invariably employed to yield alcohol by fermentation. The juice of the beet, as well as that of cane-sugar, contains, in addition to the sugar, a large quantity of extractive and nitrogenous matters, together with considerable quantities of alkaline salts. In some sugar-producing districts the waste liquors or spent wash from the stills—called *vinasses* in French—are wastefully and ignorantly thrown away, instead of being returned to the land as a fertiliser, and thus the soil becomes impoverished. In France it has long been the custom of the distiller to evaporate these liquors (*vinasses*) to dryness, and to calcine the mass in a reverberatory furnace, thus destroying the whole of the organic matter, but recovering the alkaline salts of the beet-root. In this way 2000 tons of carbonate of potash are annually produced in the French distilleries. For more than thirty years the idea has been entertained of collecting the ammonia-water, tar, and oils which are given off when this organic matter is calcined, but the practical realisation of the project has only quite recently been accomplished, through the persevering and sagacious labours of M. Camille Vincent, of Paris. The following is an outline of the process as carried out at the large distillery of Messrs Tilloy, Delaune, and Co., at Courrières. The spent wash having been evaporated until it has attained a specific gravity of 1·31, is allowed to run into cast-iron retorts, in which it is submitted to dry distillation. This process lasts four hours; the volatile products pass over, whilst a residue of porous charcoal and alkaline salts remains behind in the retort. The gaseous products given off during the distillation are passed through coolers, in order to condense all the portions which are liquid or solid at the ordinary temperature, and the combustible gases pass on uncondensed and serve as fuel for heating the retorts. The liquid distillate from the spent wash may be divided into—

1. The ammonia water.
2. The tar.

The ammonia water of the vinasse resembles that of the coal-gas manufacture in so far as it contains carbonate, sulphydrate, and hydrocyanide of ammonia; but it differs from this (and approximates to the products of the dry distillation of wood) by containing in addition methyl alcohol, methyl sulphide, methyl cyanide, many of the members of the fatty acid series, and, most remarkable of all, *large quantities of the salts of trimethylamin*. The tar, on re-distillation, yields more ammonia water, a large number of oils, the alkaloids of the pyriden series, solid hydrocarbons, carbolic acid, and, lastly, a pitch of fine quality. The crude alkaline aqueous distillate is first neutralised by sulphuric acid, and the saline solution evaporated, when crystals of sulphate of ammonia are deposited; and these, after separating and draining off, leave a mother-liquor, which contains the more soluble sulphate of trimethylamin. During the process of concentration, vapours of methyl alcohol, methyl cyanide, and other nitriles are given off, these being condensed, and the cyanide used for the preparation of ammonia and acetic acid by decomposing it with an alkali. Trimethylamin itself is at present of no commercial value, but M. Vincent finds that the hydrochlorate of trimethylamin, when heated to a temperature of 260°, decomposes into (1) ammonia, (2) free trimethylamin, and (3) chloride of methyl, and both ammonia and chloride of methyl are substances possessing a considerable commercial value. The latter compound can be employed as a means of producing artificial cold, and for preparing methylated dyes, which are at present costly, inasmuch as they have hitherto been obtained by the use of methyl iodide, an expensive substance. As a means of producing low temperatures chloride of methyl will prove of great service both in the laboratory and on a larger industrial scale, When the liquid is allowed to escape from the receiver into an open vessel it begins to boil, and in a few moments the temperature of the liquid is lowered by the ebullition below – 23°, the boiling-point of the chloride. The liquid then remains for a length of time in a quiescent state, and may be used as a freezing agent. By increasing the rapidity of the evaporation by means of a current of air blown through the liquid, or better by placing the liquid in connection with a good air-pump, the temperature of the liquid can in a few moments be reduced to – 55°, and large masses of mercury easily solidified. M. Vincent has recently constructed a freezing machine which will probably compete favourably with the ether and sulphurous acid freezing machines now in use, as it can be simply constructed, and as the vapour and liquid do not attack metal and are non-poisonous, the frigorific effects which it is capable of producing being moreover most energetic. The second and perhaps more important application of methyl chloride is to the manufacture of methylated colours. The application of methyl chloride to the preparation of violets and greens is not, however, due to M. Vincent. His merit is in establishing a cheap method by which perfectly pure chloride of methyl can be obtained, and thus rendering the processes of the manufacture of colours much more certain than they have been hitherto. Dr. Roscoe concluded his discourse by referring to this as another instance of the utilisation of waste chemical products and of the preparation on a large scale of compounds hitherto known only as chemical rarities.

NOTES.

BIOLOGY.

In a paper on the development of the ovaries in female mammals after birth, read before the Academy of Sciences, M. Ch. Rouget states that his researches have led him to conclusions agreeing with those of Kölliker. His observations support the theory of primordial hermaphroditism.

M. Chatin describes, in the "Comptes Rendus," the special nutritive apparatus of phanerogamic parasites. He finds that the suckers of such parasites are morphologically and physiologically analogous to the roots of ordinary plants.

Sarracenia purpurea contains a proximate principle closely analogous to that of the Colchicaceæ, although belonging to a remote family bordering on the Papaveraceæ.

M. Maxime Cornu points out the existence in the Crassulaceæ of cortical woody bodies agglomerated together, and apparently serving to strengthen the fragile stems destined to support an efflorescence consisting of numerous flowrets.

M. Dareste has observed, in the yolk of egg, certain granules which have generally been supposed to be lecithine, but which in their physical and chemical characters agree rather with starch.

M. Couty has made known the results of observations on the non-excitability of the grey cortical substance of the brain, which, he finds, takes no part in the phenomena produced by exciting the surface of the brain. These phenomena remain the same whether the grey layer is intact or paralysed by an anæsthetic.

M. Arm. Moreau infers, from experiments performed on dogs, that the analogy supposed to exist between the phenomena induced by the presence of certain saline solutions in the intestinal canal and the phenomena which have been studied outside the animal system by the aid of the endosmometer cannot be maintained.

MM. Bergeret and Moreau propose water slightly acidulated with nitric acid as a remedy for the *Peronospora* infesting the lettuce.

M. A. Certes recommends the vapours of osmic acid for the preservation of Infusoria.

According to M. S. Jourdain, in *Arion rufus* the arterial blood issues into the visceral cavity from funnel-shaped orifices at the extremities of the minute arteries, and is afterwards taken up by the veins, there being—as M. Milne-Edwards has shown to be the case in various regions of the body of the Mollusca—no system of capillaries establishing a continuity between the arterial and the venous systems. Similar orifices exist in many other Mollusca.

According to the "Revue Internationale des Science" a species of tortoise is found in China in which the head, neck, legs, &c., are covered not with scales, but with long and dense hair. The locality of this singular being is a lake in the province of Kiang-su. It is from 7 to 8 centimetres in length, with a prolonged snout, and a tail long and narrow.

Prof. Naegeli, of Munich, maintains that infectious and malarious diseases are due to *Schizomycetes*, which, however, are not all specifically distinct, but represent forms of one, or at most of a limited number of species.

M. P. Arnoux has brought to France a specimen of honey found in certain subterranean cavities in Ethiopia, and said to be collected by an insect "resembling a large mosquito." It is unaccompanied by wax, and differs from ordinary honey by being free from cane-sugar. We can scarcely believe that it is elaborated by a dipterous insect.

M. Ch. Musset has described to the Academy of Sciences a shower of sap which he observed, on the 22nd of August last, falling from two trees of *Abies excelsa*. Clouds of insects had collected to the feast.

M. Jousset de Bellesme has shown that the so-called liver of the Cephalopa presents no functional analogy with the liver of the Vertebrates. It transforms albuminoid matters, but is without action on fatty and starchy bodies. The author has already demonstrated the same fact concerning the liver of Crustaceans, and M. Plateau has reached a similar conclusion regarding the Arachnidæ and Myriapoda.

M. Merget, of Bordeaux, finding that mercurial vapour easily permeates disks of wood, recommends it as a means of studying the structure of vegetable matter. If wood, after exposure to the vapours of mercury, is brought in contact with a sensitive paper (obtained by saturating paper with an ammoniacal solution of nitrate of silver) a distinct design of the fibro-vascular bundles and of the medullary rays will be obtained. We may thus design the stomata of a leaf, and show that in case of those possessing stomata on both surfaces the air circulates from one epidermis to the other.

CHEMISTRY AND TECHNOLOGY.

Mr. A. Percy Smith communicates to the "Chemical News" a case of poisoning by arsenical wall papers. A lady in Rugby complained of a peculiar feeling of constriction in her nose, accompanied by a "nasty smell," nausea, and loss of appetite, and was unable to discover the cause. The symptoms had been felt since sleeping in a certain room, which she had done for two months. Mr. Smith analysed the paper, and found 0·35 grm. As_2O_3 per square foot; but that scarcely represents the total amount, because the paper was wetted in order to remove it, and the greater proportion of the bright green flowers washed off. There was very little copper present. The lady's husband was not affected in the slightest degree.

Writing to the "Chemical News" on the analysis of Australian *Eucalypti*, Mr. R. D. Adams, of Sydney, N.S.W., says that the celebrated *Eucalyptus globulus* is by no means the richest in the essential oil which gives the peculiar sanitary value to this class of trees, although it is the most "taking" in its earlier stages, from the rapidity of its growth and fulness of foliage. It is the *E. amygdalina* which yields more volatile oil than any other tested, and which therefore is largely chosen for distillation; thus it is also one of the best for subduing malarian effluvia in fever regions, although it does not grow with quite the same ease and celerity as *E. globulus*. The respective hygienic value of various Eucalypts may to some extent be judged from the percentage of oil in their foliage, as stated below, and as ascertained by Mr. Bosisto at the instance of Baron Ferdinand von Mueller, Director of the Victorian Botanical Department for the Exhibition of 1862:—

E. amygdalina	3·313	per cent	volatile oil.
E. oleosa	1·250	,,	,,
E. leucoxylon	1·060	,,	,,
E. goniocalyx	0·914	,,	,,
E. globulus	0·719	,,	,,
E. obliqua	0·500	,,	,,

The lesser quantity of oil of *E. globulus* is, however, compensated for by the vigour of its growth and the early copiousness of its foliage. The proportion of oil varies also somewhat according to locality and season. *E. rostrata*, though one of the poorest in oils, is nevertheless important for malarian regions, as it will grow well on periodically inundated places, and even in stagnant water not saline. *E. oleosa* (F. v. M.), from the desert regions of extra-tropic Australia, might be reared on barren lands of other countries for the sake of its oil. According to Mr. Osborne's experiments, *Eucalyptus* oils dissolve the following, among other substances, for select varnishes and other preparations:—Camphor, pine-resins, mastich, elemi, sandarac, kauri, dammar, asphalte, xanthorrhæa-resin, dragon's blood, benzoe, copal, amber, anime, shellac, caoutchouc, also wax, but not gutta-percha. These substances are arranged here in the order of their great solubility. The potash obtainable from the ashes of various

Eucalypts varies from 5 to 27 per cent. One ton of the fresh foliage of *E. globulus* yields about 8½ lbs. of pearlash; a ton of the green wood, about 2¼ lbs.; of dry wood, about 4½ lbs.

In a petition to the Reichstag concerning the use of alleged poisonous colours, the Association for Promoting the Interests of German Chemical Industry point out that certain dyes, such as red corallin and aurantia, denounced as poisons, have subsequently been proved perfectly innocent. They pray that no colour may be prohibited till it has been formally pronounced dangerous to public health by a commission of specially qualified chemists and physiologists after a due scientific examination.

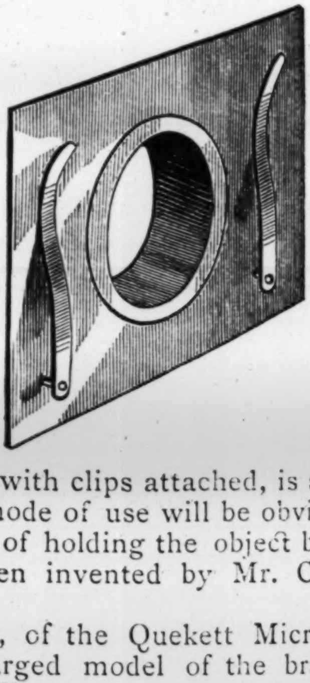
While engaged in preparing a Supplement to his "Handbook of Chemical Manipulation, Mr. Greville Williams, F.R.S., made inquiries with the view of ascertaining what improvements had recently been made in the volumetrical determination of acids and alkalies. Amongst the persons consulted was Dr. Otto N. Witt, who pointed out that the "orange 3" of Porrier formed an excellent substitute for litmus. This new coal-tar derivative was introduced into commerce about two years ago: its manufacture has, however, been almost entirely, if not quite, abandoned in favour of "orange 4" or tropæolin OO. This "orange 3" had, before its introduction into commerce, been studied independently by Dr. Witt and M. Griess; and M. Lunge, in the "Berichte der Deutschen Chemischen Gesellschaft," expressly mentions it as an indicator. Its chemical name is dimethyl-amido-azo-benzol-sulphonate of ammonium. Different specimens of litmus somewhat vary in their sensitiveness to acids and alkalies; that with which Mr. Williams experimented in comparing "orange 3" and litmus ceased to indicate with sulphuric acid when more dilute than 1 to 50,000. The new indicator, on the other hand, remained sensitive to 1 part of sulphuric acid in 100,000 of water. An immense advantage which this indicator possesses over litmus is that it is entirely unaffected by carbonic acid; the solution need not therefore be boiled, and the operation consequently takes much less time; in fact, four operations can be made in the same time as one with litmus. Mr. Williams also finds that the new indicator can be employed in the determination of ammonia instead of litmus, and has many advantages over it.

M. G. Luvini has preserved the eggs of the silkworm in hydrogen, carbonic acid, oxygen, and nitrogen for about three months. Those which had been kept in carbonic acid and in nitrogen after removal hatched well and gave an almost complete yield.

PHYSICS.

A galvanometer, which measures the strength of the electric current directly, instead of indirectly, as in the instruments commonly employed, has, we learn from the "Engineering and Mining Journal," been constructed by Prof. Dolbear, of Tufts College. He utilises the force exerted by a common helix to draw the core within itself when a current is passed through it, which is directly proportional to the strength of the current. A simple mechanism, consisting of a spiral spring, a graduated scale, and a pointer, suffices to measure the strength of the current.

A simple contrivance for holding the object beneath the stage of the microscope, when extreme obliquity of illumination is required, has been devised by Mr. John Phin, of New York, and has the advantage of being easily adapted to any microscope.



The little sub-stage, with clips attached, is slid into the aperture in the stage. The mode of use will be obvious. Mr. Phin also states that the plan of holding the object beneath the stage is not new, having been invented by Mr. C. A. Spencer about twenty years ago.

Mr. E. J. Newton, of the Quekett Microscopical Club, has constructed an enlarged model of the brain of a cockroach (*Blatta orientalis*). The brain, properly hardened, was cut into sections of 1-1000th of an inch in thickness, and after the sections were mounted a careful outline of each was made by the camera lucida, on a piece of soft pine-wood of suitable thickness. Each slice was carefully cut out with a fine saw, and the whole piled together to form the magnified model. The surface of each section was carefully coloured, after the original preparations, to represent the appearance presented under the microscope.

The "Journal of the Royal Microscopical Society" has completed its first annual volume. The Fellows may be congratulated upon the improved form their official publication has taken. The volume contains 402 pages, comprising the whole of the papers read before the Society, seventeen plates besides woodcuts in the text, full reports of the meetings, translations of microscopical and biological papers from foreign sources, and a list of similar articles in several British and foreign publications. Mr. Frank Crisp, LL.B., B.A., F.L.S., one of the Secretaries of the Society, has undertaken the honorary editorship.

The Council of the Royal Microscopical Society have devised a plan by which other societies founded for kindred objects should be brought into association with them. The presidents for the time being of such societies, at home or abroad, as the Council may from time to time recommend, shall be *ex officio* Fellows, and shall receive the publications of the Royal Microscopical on behalf of their respective societies, and exercise all other privileges of Fellows, voting excepted. At a recent meeting the Quekett Microscopical Club, the South London Microscopical Club, the Croydon Microscopical Club, and a large number of provincial and foreign societies, were, through their respective presidents, admitted to *ex officio* fellowship.

The microscopic phenomena of muscular contraction has recently been studied by Th. W. Engelmann: the results are given in "Archives Neerlandaises." During the contraction of the transversely striated muscular fibres there are produced, parallel with the change of form of the muscular elements, changes in the optical properties of the isotropic and anisotropic layers. These changes are of an opposite nature in the two layers, the isotropic stratum in its totality becoming more refrangent and the anisotropic less refrangent. In consequence, at a certain degree of shortening, the fibre seen by common light may appear homogeneous without appreciable transverse striæ—the stage of homogeneity or of transition. If the shortening is carried further the transverse striæ corresponding to the isotropic disks are seen to reappear. At any given phase of the process of contraction, consequently even in the transition stage, the isotropic and anisotropic substances may be recognised by means of the polarising microscope as well-defined and regularly alternating strata. They do not, at the time of contraction, exchange their respective places in the muscular compartment. The thickness of both strata decreases during contraction, that of the isotropic layer much more rapidly than that of the anisotropic. The total volume of each compartment does not vary sensibly during contraction. The anisotropic strata increase in volume at the expense of the isotropic, as during contraction liquid passes from the latter to the former.

Some interesting researches on electric fishes have been communicated to the French Academy of Sciences by E. J. Marey. Physiologists have been struck with the analogies presented by a muscle and the apparatus of electric fishes. These two kinds of organs, both subject to the will and provided with nerves for centrifugal action, have further a very analogous chemical composition, and even some points of structural resemblance. These views, put forward before physicists had developed the theory of the correlation of forces, were necessarily very vague. It might even be conceived that in the living organism, as well as in our physical instruments, analogous conditions might produce either mechanical work or electricity. Having found that muscular acts are complex, *i.e.*, that a muscle in tetanus or in contraction executes a series of minute successive movements, which M. Marey calls *shocks*, which accumulate to produce muscular attraction, he has examined the discharge of the torpedo, and found there a similar complexity. Having passed this discharge through an electro-magnetic tracing apparatus, he found that it was made up of minute shocks, which recur at the rate of 150 in a second. Cold reduces the rate alike of the muscular and electric shocks, whilst heat acts inversely. Hence M. Marey concludes that these two functions are really homologues. The gymnotus gave similar results to those of the torpedo. The latter fish, when connected with a telephone and slightly excited, produced a very short croaking. If a prolonged discharge is occasioned by pricking the electric lobe of the brain, the sound produced lasts three or four seconds, and in tonality borders upon *mi* (165 vibrations).

On examining with the spectroscope, both by absorption and by means of the electric spark, the products of his operations on the mixture of earths from samarskite, M. Lecoq de Boisbaudran has observed rays or bands not to be referred to any element formerly known, and not corresponding to the descriptions of the spectra of the earths recently announced by MM. Delafontaine, L. Smith, Soret, and De Marignac. These new rays of absorption and emission seem to belong to one and the same body. The emission spectrum is composed of four bands shaded towards the left and formed of narrow rays, the strongest of which is the most refrangible and forms the right margin of the band. The absorption spectrum comprises two strong bands in the blue, and several rays of less importance in the green. The metal which yields these new spectra is precipitated as a double potassic sulphate along with didymium; its simple sulphate is rather less soluble than that of didymium; its oxalate is precipitated along with didymium, but ammonia separates the oxide of the new metal before that of didymium.

An improvement in Bunsen's battery is described in "Les Mondes" by M. Lefebure. The oxidation of the exterior surface of the zincs contributes nothing to the liberation of electricity, so it may be safely covered with varnish, thus reducing the consumption of zinc by one-half.

PHILOSOPHICAL SOCIETY

THE MONTHLY

JOURNAL OF SCIENCE.

JUNE, 1879.

I. COLOUR AND ITS RECOGNITION.[*]

It is not without some reluctance that we venture to reopen a question which has been abundantly handled both by workers and dreamers, and which, though by no means decided or exhausted, must await further discoveries for its ultimate solution. We wish, however, to lay before our readers certain speculations calculated to prove valuable and suggestive not merely to professed men of science, but also to the cultivated lay-public. Although disclaiming all right to rank as a biological specialist, Mr. Grant Allen, the author of the work before us, has, in our opinion, thrown a welcome and seasonable light upon some of the most obscure features of the animal and vegetable world. Even though certain of the conclusions to which he has led may not be accepted as final, they must surely give a new stimulus and a fresh direction to research.

Mr. Allen's immediate object is to prove that the colour-sense—so far from being, as Dr. Magnus and Mr. Gladstone have on mere philological grounds sought to demonstrate, a recent development of the human mind—must have existed in pre-historical ages, and that it may be traced far down the animal series. In opposition, again, to a far more formidable authority, Mr. A. R. Wallace, he defends the theory of Sexual Selection. In the very outset of his work he suggests the law that "only those animals display beautiful colours, due to Sexual Selection, in whom a taste for colour has already been aroused by the influence of flowers, fruits, or brilliant insects, their habitual food,"—a principle which we shall have to examine in some detail. He shows that, practically speaking, our idea of colour corresponds with that of the lower animals. He traces a connection between sight and the power of locomotion, contending that this sense is most developed among winged creatures.

---

[*] The Colour-Sense, its Origin and Development: an Essay in Comparative Psychology. By Grant Allen, B.A. London: Trübner and Co.

We find it, then, asserted, that a perception and taste for colour is first aroused in animals by the influence of the objects on which they feed. On the other hand, the disciples of the new school of Natural History—and no one more emphatically than our author—contend that it is to the colour-sense, especially of insects and birds, that we owe all the varied colouration of blossoms and fruits as distinct from the monotonous green of earlier geological epochs. We can admit that, in virtue of the principle of Natural Selection, the smallest step on the part of a plant to produce a gay efflorescence, or the smallest step on the part of an insect towards the appreciation of a source of edible pollen or saccharine matter, would, *pro tanto*, give the advantage in the struggle for existence to the species in which it might occur. But where and how was the first step taken? "Did flowers show an original tendency to the production of coloured adjuncts prior to the selective action of insects? Did insects possess any tendency vaguely to discriminate colours apart from the reactive influence of entomophilous flowers?" Mr. Allen, in addressing himself to these questions, argues that the normal green colour of plants is connected with de-oxidation, and the consequent storing up of energy. But where oxidation and the expenditure of energy are known to be in progress,—as in decaying leaves, in buds, young shoots, and sprouts, especially those put forth in the dark, where the reductive action of the solar rays is of course wanting,—we find a rich and varied display of colour. The author gives as instances the sprouts of peonies, which are of a full dark red: the rosy stems and yellow early leaves of rhubarb, &c. Now all these colours are indeed merely adventitious. But in flowers, where the colouration is a "purposive adaptation," we find, even in anemophilous species, a rise of temperature, from which we know that oxidation is in progress. Hence we may naturally expect that all floral organs, whether anemophilous or entomophilous, would have a tendency to the production of bright colours. This is accordingly seen even in the mosses, lycopods, and ferns. We have thus a groundwork of differentiated colouring upon which Natural Selection or any other agency may operate. The author accordingly decides that "the bright pigments of entomophilous plants are due originally to natural oxidation aided by the selective action of insects." To the second question, whether insects had any tendency to discriminate colours apart from the reaction of entomophilous flowers, he replies that colours differ not merely qualitatively, but quantitatively, according as they absorb a greater or less proportion of the luminous rays. Thus "white might be distinguished by the primitive eye from green, brown, or black." If a beginning is thus given the rest will easily follow.

In a chapter devoted to the colour-sense of insects, the reality of which he proves by an appeal to numerous and fully-established facts, the author reverts to the views of Mr. Gladstone, Dr. Geiger, and Dr. Magnus, and declares that they have been "partially adopted" even by Mr. Wallace in his "Tropical Nature." This we can scarcely admit, since in that very work he points out the cardinal error in Mr. Gladstone's reasoning, viz., that the absence of a precise nomenclature for colours is no proof of the lack of colour-perception. In disposing of the old theory of the colouration of flowers, our author happily remarks:—"Not even the watch-maker deity of Paley himself, one may suppose, would have invented flowers in the Secondary Age for the sole gratification of man in the Post-tertiary. To put it briefly, if insects have not a colour-sense, then the whole universe must be nothing more than a singularly happy concourse of fortuitous atoms."

Just as the varied hues of flowers are due to the selective action of insects, so the colouration of fruits must be sought in the reaction of birds and mammals. These, attracted by the colour no less than the odour of fruits, swallow the pulp, and void the hard, smooth, indigestible seeds with their excreta, placing them thus under highly favourable circumstances for germination. Hence the plant whose fruits assumed the most brilliant and striking colouration would gain a decided advantage in the struggle for existence.

On the fact that some poisonous, indigestible, or otherwise non-eatable fruits display a brilliant colouring much might be said did space permit. The author hazards the suggestion that some plants, such as the common arum and the manchineel, may derive a benefit from the poisonous property of their seeds, which if eaten by birds or small mammals might prove fatal, and "would thus have an opportunity of germinating in the midst of a rich manure heap formed of its decomposing body." We must remember, however, that poisonous seeds are avoided by most species and prove harmless to others.

Among the Mammalia, the author declares that the evidences of a colour-sense are mainly wanting. "The antipathy of male ruminants for scarlet," which he mentions,* seems to us to point to a tolerably well-developed recognition of colour. The bull, *e.g.*, must be able to discriminate between scarlet or blood-red and that reddish brown so common in his own species. If, however, the author's theory be correct, an absence of the colour-sense, save in monkeys, and perhaps squirrels, need not surprise us. No other mammals feed upon fruits or are very specially connected with flowers. This same deficiency, Mr. Allen considers, is the cause why mammals display none of the brilliant colouration so common in other departments of the animal kingdom. We cannot, however, refrain from pointing out that the golden mole, the only mammal endowed with fur of a metallic lustre, is certainly no fruit-eater.

Among flower- and fruit-feeding species in the most different groups of the animal kingdom the author considers that there prevails a community of tastes, in strongly-marked contrast to those of the Carnivora and the carrion-feeders. With this view, however, we cannot entirely agree. Many of the perfumes which are most agreeable to us are equally attractive to cats. Nothing seems to delight a leopard more than a saucer of lavender-water, and the common cat rolls in delight upon a number of plants which secrete essential oils, and which certainly have no connection with her ordinary diet.† On the other hand, many butterflies, though feeding ordinarily upon the nectar of flowers, may be attracted by odours of a most antagonistic character. Perhaps the easiest way to capture high-flying species, such as *Apatura Iris*, and the splended *Papilios* and *Ornithopteras* of sunnier climates, is to bait for them with excrement or carrion, such as a dead rat or a weasel. Many of our common native butterflies may be seen haunting manure-heaps and sipping the fœtid moisture.

To a certain passage in Mr. Wallace's "Tropical Nature,"‡ in which this illustrious biologist considers that an insect's capacity to distinguish colours may probably be quite unaccompanied by a sense of radical distinctness,

---

* Is this antipathy confined to males? Mr. Allen elsewhere remarks that dogs are attracted by bright colours.

† "Senses of the Lower Animals." (Quarterly Journal of Science, vol. viii., p. 297.)

‡ Page 238.

Mr. Allen takes exception in a passage which closely coincides with our remarks on the same subject.*

In a chapter on the "Direct Reaction of the Colour-Sense upon the Animal Integuments" the author proceeds to the demonstration of what we may call his initial law. He seeks to show, by a multitude of instances, that the most beautiful insects and birds are such as haunt flowers and fruits, either feeding directly upon their pollen, nectar, sweet juices, &c., or preying upon minute insects attracted there for the same purpose. He cites the observation repeatedly made by travelling naturalists, that where the flora is most beautiful the fauna is correspondingly splendid, whilst if the one is dingy and insignificant the same characters apply to the other. Exceptions will doubtless occur to the mind of the naturalist, especially if he be a student of insect-life. Thus though the author remarks, with much general truth, that "first in order of ugliness must be placed the carrion-feeders who live upon decaying bodies or animal excrements," yet if we look over a tolerably complete assortment of beetles of the great neo-tropical genus *Phaneus*—all, be it remembered, dung- and carrion-devourers—we find almost every variety of rich iridescent and metallic hues, emerald-greens, golden and ruddy bronzes, gorgeous blues, and purples. Great splendour may also be found among the *Coprides*, another group of filth-devourers; and even our common English dor-beetles show on their under surfaces pure and beautiful metallic hues. Sometimes, again, one and the same pattern or arrangement of colour may be traced in a number of species belonging to distinct groups, and feeding on substances of a totally different nature. For instances of this kind the reader is referred to the "Monthly Journal of Science" for February (p. 196). To the cases there mentioned may be added the death's-head moth, the convolvulus-hawk moth, &c. Mr. Allen himself refers to the tiger-beetles as exquisitely beautiful and highly carnivorous; and many other rapacious beetles, of the genera *Carabus*, *Calosoma*, *Pœcilus*, &c., are remarkable for the loveliness of their tints. Still we do not feel warranted in concluding that these apparent exceptions, and others which might be adduced, justify the rejection of Mr. Allen's law. Our knowledge of the diet and habits of insects is still very imperfect. Perhaps, too, the cases we have mentioned are "residual phenomena," which on further study will point the way to some further generalisation.

---

* Quarterly Journal of Science, vol. viii., p. 459.

In speaking of the generation of pigments Mr. Allen quotes with approval the declaration of Mr. Lowne,* that these substances are apparently due to the waste-products of other organs, and do not take away anything from the effective energies of the system. It might, perhaps, be asked how this view agrees with the one put forward in an earlier part of his work, where the production of colour is connected with the expenditure of energy? We fully admit the very high proportion of nitrogen present in feathers, and we recognise the fact that those animals—insects, birds, and reptiles—which are most richly coloured excrete uric acid to a notable extent. At the same time it must be admitted that the most beautiful colouring of such animals is due not to any pigment, but to the interference of light arising from the micro-mechanical texture of the feathers, wing-scales, elytra, &c.

Concerning "prohibitive" or "warning" colour, the author declares himself somewhat sceptical. As regards the eggs of birds he suggests that "the colouration may act as a supplementary allurement to the instinct of incubation." But how if the nest, as occurs in not a few species, such as the kingfisher, is absolutely dark within?

The concluding chapters of the work treat of the colour-sense in man, of the esthetic value of colour, and of the growth of the colour-vocabulary. By testimony collected through the aid of consuls, missionaries, and others, from semi-civilised and savage tribes, it is shown that the colour-sense is universal. By the evidence of gems and glass and stone beads, obtained from the lake-dwellings of Switzerland from barrows of the Stone Age, and of pottery from Dr. Schliemann's excavations at Mycæna, a cumulative proof is furnished that this same sense is of no recent growth.

---

* Philosophy of Education, p. 75.

II. EARLY TRACES OF MAN.*

By G. De Mortillet.

QUATERNARY MAN.—The man of geological time—fossil man—is now a fact so clearly demonstrated that it is no longer called in question. The recent exposition of anthropological sciences showed us his works plentifully scattered throughout France, England, Spain, and Italy.

But, though the existence of quaternary man in the south-west of Europe is no longer denied, there is a school which, walking with fear and hesitation in the path of progress, has its mind made up to contest his existence in the Orient. What the leaders of this school maintain is this:—In the East, say they, civilisation, and consequently historic records, date back to a very remote time. Is it not, then, possible that geological time still persisted in Europe, and especially in Western Europe, while in Egypt the historic dynasties were being founded?

To put forth such a proposition as this, one must be ignorant of the data of geology. The remarkable collections exhibited at the Anthropological Exposition have shown that man was contemporary not only with the reindeer, the saiga, the chamois, and the marmot on our plains; not only with the mammoth and the *Rhinoceros tichorhinus*,—that is, with the fauna of the Glacial period, but also with the great hippopotamus, the *Elephas primigenius*, and the rhinoceros of Merk. All geologists are agreed that the duration of the period in which we live is as nothing compared with that of the Quaternary period. It is as a day compared to ages, as a drop of water in a stream. All palæontologists understand what a length of time is requisite for the rise and decline of animal species—species which, while they have been upon the earth, have been lavishly distributed over an enormous area.

But we have no need of the general data of geology and palæontology in order to meet the objection. The Exposition of the Anthropological Sciences furnished materials which reduce it to a nullity. There were exhibited perfectly characterised quaternary instruments of silex from the East

---

* Translated by J. Fitzgerald, A.M., from the Revue d'Anthropologie.

—from the most ancient seats of civilisation, Egypt and Syria. In those countries, then, no less than in France and England, quaternary man preceded all the historic civilisations.

The earliest Quaternary epoch, the preglacial, is characterised, so far as man's works are concerned, by a stone implement of peculiar form. It is dressed on its two sides, usually rather roughly chipped; it is rounded at the base, pointed at the top, and its edges are pretty sharp. In general form it is more or less almond-shaped. This implement, in past times called by workmen in quarries "langue de chat" (cat's tongue), is now called "hache de St. Acheul," or "hache acheulienne" (hatchet of St. Acheul), terms derived from the locality in which it has been oftenest found. They have been found in abundance in the quaternary alluviums of France, England, and Spain. Nay, within a few years they have been found in the valley of the Delaware near Trenton, New Jersey, by Dr. Charles C. Abbott. The figures which he has published, and his descriptions, tally exactly with the St. Acheul hatchets of France and England.

Nor is it in the New World only that the existence of man in the earliest portion of the Quaternary period has been proved; the same thing is true of the Old World. M. Place, the explorer of Assyria, has brought to light a St. Acheul hatchet of silex which he found under the ruins of the palace of Khorsabad. At the Exposition the Abbé Richard showed a St. Acheul hatchet, also of silex, from the Lake of Tiberias.

A still more conclusive proof is furnished by Prof. Henry W. Haynes, of Boston, who reports a number of wrought flints from Egypt, among them several clearly characterised St. Acheul hatchets.

In the February number (1869) of the "Matériaux pour l'Histoire de l'Homme," M. Adrien Arcelin first made the announcement that the grand Egyptian civilisation, like all other civilisations, was preceded by an age of stone. He had just collected in Upper Egypt several clipped flints. Toward the close of the same year this discovery was confirmed by Messrs. Lenormant and Hamy. All the specimens brought home by these earliest explorers might be regarded as belonging to the Robenhausen epoch, or age of polished stone; only one specimen, presented to the museum of St. Germain, came anywhere near the St. Acheul type.

After Arcelin's discovery, collections of dressed flints were multiplied in Egypt, though without throwing much light upon the question. But Sir John Lubbock, in an essay illustrated with fine plates, gave figures of three flint implements found at Luxor and at Abydos, which are undoubtedly St. Acheul hatchets.

Among the wrought flints brought from Egypt and exhibited by Mr. Haynes are several which incontestably are of the quaternary type. Among them we see scrapers and arrow-heads, the latter belonging to a type which in France occurs only in glacial formations. The collection also embraces more ancient forms, preglacial forms, referable to the early portion of the Quaternary period, viz., St. Acheul hatchets of flint.

These St. Acheul hatchets come from two very distinct localities: one lot is from the neighbourhood of Luxor, in Upper Egypt; the other from the environs of Cairo, in Lower Egypt. The flint used, as is clearly proved by Delanoue, comes from the nummulitic formations. These formations are found *in situ* in Upper Egypt; and the St. Acheul hatchets of that region are as a rule heavier and better wrought,—above all, more completely wrought. In the environs of Cairo there are no rocks *in situ;* and as for flint, only rounded nodules are found. These nodules have been wrought into the forms of implements. This is easily seen, for all the St. Acheul hatchets of that locality still bear at their base traces of the original rounded surface of the nodules.

From these archæological data—*i.e.*, from the nature and form of the objects—we may conclude that the man of the earliest Quaternary times lived in Egypt simultaneously with his existence in Europe, and that in both of these regions his industrial development was about the same, extremely primitive. And geological observation confirms these deductions. It was not on the surface of plateaus that Mr. Haynes found these St. Acheul implements. On the contrary, most of them, at least from the neighbourhood of Luxor (forming the greater number), were found in the bottom of the ravines of Bab-el-Moluk. These ravines are cut deep into the quaternary deposits by the torrents which, in seasons of heavy rainfall, carry to the Nile the waters from the mountains of Libya.

Thus, then, thanks to the Exposition of the Anthropological Sciences, we are in a position to show that the oldest Egyptian civilisation,—that of the earliest dynasties,—which dates back 4000 years before our era, was preceded by an age of polished stone, and that before that period Egypt, like all the rest of the world, was occupied by quaternary man.

Tertiary Man.—Important as are the results of the Anthropological Exposition from the point of view of quaternary man, they are still more so from the point of view of tertiary man.

But first let us understand what is meant by the terms quaternary and tertiary man.

The fauna of the mammals serves clearly to determine the limits of these later geological periods.

The Tertiary is characterised by terrestrial mammals entirely different from extant species; the Quaternary by the mingling of extant with extinct species; the present period by the extant fauna.

The man of the early Quaternary, he who made the St. Acheul hatchets and used them, is the man of Neanderthal, of Canstatt, of Enggisheim, of La Naulette, of Denise. He is indubitably a man, but differing more widely from the Australian and the Hottentot than the Australian and Hottentot differ from the European. Hence unquestionably he formed another human species, the word species being taken in the sense given to it by naturalists who do not accept the transformation doctrine.

Tertiary man, therefore, must have been still more distinct—of a species still less like the present human species; indeed, so different as to entitle it to be regarded as of distinct genus. For this reason I have given to this being the name of man's precursor. Or he might be called anthropopithecus—the man-monkey.

The question of Tertiary man should therefore be expressed thus:—Did there exist in the Tertiary age beings sufficiently intelligent to perform a part of the acts which are characteristic of man?

So stated, the question is settled most completely by the various series of objects sent to the Anthropological Exposition.

The first and oldest of these collections was that made by the late Abbé Bourgeois, at Thenay (Loir-et-Cher). At the International Congress of Prehistoric Archæology and Anthropology, held in Paris in 1867, the Abbé Bourgeois exhibited tertiary flints which, he claimed, had been chipped intentionally. These early specimens were not very conclusive, lost as they were amid a multitude of other specimens which certainly had not been fashioned intentionally, unless one can suppose that they had been intentionally split by the action of fire. The result was that the Abbé's communication won to his side but few adherents. But, profoundly convinced of the reality of his discovery, the Abbé Bourgeois did not lose heart on suffering this partial repulse. He continued his researches with vigour, and again, in 1872, provided now with better specimens, he raised the question at the Brussels Congress. There he made some headway among the best experts. But on the commission which was specially appointed to examine the flints were several members who knew but very little directly about the manner of working on flint, and they either hesitated or passed an adverse judgment. Hence the question was not definitively settled. This result—half success, half failure—stimulated the ardour of the accomplished naturalist; he continued his investigations, and so succeeded in collecting for the Anthropological Exposition a remarkable series of flint implements which dispels all doubt.

This collection was made up of flints which beyond a doubt had undergone the action of fire. They are full of cracks, and even quite discoloured. With these are other flints, far more numerous, which have simply been split by fire. Among them are some which unquestionably have been neatly and regularly retouched on one or both of their margins. Every one who has carefully and impartially examined them has admitted that the second dressing (*les retailles*) was certainly intentional, and consequently that it was the work of an intelligent creature.

It remains to determine the age to which these flints belong. They were collected at Thenay, in formations clearly *in situ* and intact, and belonging to the formation known among geologists as "calcaires de Beauce;" but now these calcaires de Beauce constitute the lower strata of the Middle Tertiary. This is shown by the fauna which the Abbé Bourgeois exhibited in connection with the flints. This fauna, which comes from the sands of the Orléanais, which directly overlie the calcaires de Beauce, comprises great mastodons and dinotheriums belonging to the Lower Miocene. Then there is the acerotherium, a genus akin to the rhinoceros, and which was found in the very same stratum as the fire-split and re-dressed flints.

It results, therefore, from the Abbé Bourgeois's researches, that during the Middle Tertiary there existed a creature, precursor of man, an anthropopithecus, which was acquainted with fire and could make use of it for splitting flints. It also knew how to trim the flint-flakes thus produced and to convert them into tools.

This curious and interesting discovery for a long time stood alone, and arguments were even drawn from this isolatedness to favour its rejection. Fortunately another French observer, M. J. B. Rames, has found in the vicinity of Aurillac (Cantal), in the strata of the upper part of the Middle Tertiary,—here, too, in company with mastodons and dinotheriums, though of more recent species than those of Thenay,—flints which also have been re-dressed intentionally. Here, however, the flints are no longer split by fire, but by tapping. It is something more than a continuation, it is a development. Among the few specimens exhibited by M. Rames, whose discoveries are quite recent, is one which, had it been found on the surface of the ground, would never have been called in question.

The weighty facts developed by French investigators received striking confirmation in the Portuguese department of the Exposition. A distinguished *savant* of Lisbon, Senhor Ribeiro, director of the Geological Bureau of Portugal, sent a collection of flints and quartzites found in the strata of the Middle Tertiary or Miocene, and in the Upper Tertiary or Pliocene of the valley of the Tagus. Among these specimens—ninety-five in number—are twenty-two which bear unquestionable traces of intentional chipping. Nine specimens, all of flint, are described as coming from the Miocene. Of the others, purporting to be Pliocene, seven are of flint and six of quartzite. All these specimens are roughly chipped, and nearly all are triangular in form, and not re-dressed, whether the material be flint or quartzite.

Thus, then, the Anthropological Exposition, important though it was from the point of view of Quaternary man, is still more important from the point of view of Tertiary man —man's precursor. His existence can no more be denied.—*Popular Science Monthly.*

III. LEAVES AND THEIR FUNCTIONS.

By Rev. L. J. TEMPLIN, Hutchinson, Kas.

A LEAF, whatever may be its configuration or colour, is always an object of interest. But how few people, when they see a leaf as it waves and flutters in the breeze, really know what they are looking at. Leaves appear in an endless variety of forms, sizes, and colours. They are often so transformed that it is more by the place they occupy than by their forms that we know they are leaves. Underground stems or rhizomas have them at each point or node as little thin scales. Buds are enveloped in peculiar cerements, which generally fall away soon after the ordinary leaves have begun to expand; those enveloping scales are only leaves in a modified form. They are quite prominent in the hickory and horse-chestnut. The scales of bulbs, as of the lily, are simply modified leaves. Flowers are only aggregations of metamorphosed leaves. But it is with leaves as foliage that we are more immediately concerned at present. A complete leaf consists of three parts—the stalk or stem (petiole) on which it rises, the expanded blade or lamina, and two small leaf-like appendages at the base of the leaf-stalk called stipules. The only essential part is the blade, as this may be sessile on the stem without petiole or stipules. The blade of a leaf consists of three portions:—the woody framework, ribs, or veins; the green cellular portion, pulp; and the outside covering, or epidermis. The epidermis, which is really an extension of the outer bark of the stem, is composed of closely united, transparent cells, with frequent openings through it called stomata or breathing-pores. These vary in number from 800 to 170,000 to the square inch of surface. It is through these that water is exhaled from the plant. They are more numerous in the leaves of plants growing in moist situations and surrounded by a damp atmosphere. The pores dilate with the increase of humidity and contract with the increase of aridity. Plants growing in arid climates have but few stomata, and these are very small. While the most of foliage appears to be made on the principle of exposing the greatest possible surface to the air, some forms of vegetation seem to be constructed for the accomplishment of the very reverse of this. Thus the various species of *Cactus*, whose native habitat is the hot, arid plains of the South-west, are constructed on the principle of presenting the least extent of surface to the air, and this surface is covered with an epidermis that is almost impervious to water. This is necessary to prevent excessive transpiration in that very dry climate. The pulp or parenchyma of the leaf is made up of several layers of cells. These cells are small globular sacs, varying from 1-1200th to 1-250th of an inch in diameter. A layer of these, of a rather elongated form, is arranged immediately beneath the epidermis of the upper side of the leaf with the ends to the surface. These are crowded quite closely together. Another layer, not quite so much elongated and less compactly arranged, is found on the under side of the leaf. Between these two layers are numerous globular cells that seem thrown together without any great regularity or order. Among these are numerous irregular passages, intercellular spaces, through which water and air circulate. These reach the surface through the stomata of the epidermis. It is worthy of notice that by far the larger part of these breathing pores are on the under surface, and this surface always seems to avoid direct sunshine. If a leaf is inverted, turning the bottom side upward, it will, if possible, return to its natural position, and if prevented from so doing it will soon die. A few leaves have been known to grow in a vertical instead of a horizontal position. The framework of leaves consists of wood, and is intended to give firmness and support to the leaf. It is divided into numerous veins or nerves that ramify every part of the green parenchyma. There are two distinct systems of venation of leaves—the parallel-veined and the net-veined. In the former the fibres run nearly parallel from one extremity of the leaf to the other; such leaves are usually long and narrow, linear, as in the grasses, corn, &c. In the other the veins are netted, ramifying the leaf in all directions, and dividing the parenchyma into numerous small squares and diamonds. This style of venation exists under two forms: in one a principal vein, midrib, extends from the base to the apex of the leaf, and from this numerous smaller veins branch off and run to the margin; in the other there are three or five nearly equal ribs running the length of the leaf. The first is feather-veined, from its resemblance to a feather; and the other is palmately veined, the main ribs branching out like the fingers of a hand. The shape of a leaf is generally determined by the manner of its venation. The two principal styles of venation belong to and denote two different classes of plants, the parallel-veined belonging to the monocotyledonous, and the net-veined to the dicotyledonous divisions of the vegetable kingdom. Thus the veining of a small portion of a leaf will indicate to which of these classes the plant upon which it grew belonged.

The green colour of leaves comes from a granular substance, chlorophyll, found in the cells of the parenchyma. In its absence no true vegetable structure can be built up from the original elements, and it can operate only in the presence of sunlight. Low cryptogamic plants will grow in the dark, but they contain no proper chlorophyll. Chlorophyll has been found to be composed of two different substances,—xanthophyll, a yellow substance, and cyanophyll, a blue material: their union forms chlorophyll, or leaf-green. It is thought that the yellow colour of leaves at maturity is caused by the predominance of xanthophyll at that time. Besides chlorophyll the leaf-cells contain the proximate principles of the plant; and here the real work of building plant structure is performed. But this brings us to the consideration of the second part of our subject, viz., the functions of leaves.

In treating this branch of the subject it will be necessary to consider the leaf under several different characters. Leaves should be considered as real living beings, capable of performing vital functions—as workers performing a large amount of important work. We may first consider *the leaf as a pump*. One of its most important offices is to pump up water from the soil through the roots and stems of plants. This it exhales through its stomata in the form of invisible vapour. By this means a large quantity of water is carried up from the soil to the atmosphere. Thus a large portion of water that would quickly settle down through the deeper soil, and find its way into underground passages, is carried up and given off to the atmosphere, where it is condensed into clouds and descends in rain, thus watering and making fruitful the earth. Without this work many parts of the earth that now blossom as the rose would become arid wastes. The amount of moisture thus carried up and exhaled by the foliage of trees and plants is immense. A sunflower, with a leaf surface of 39 square feet, exhaled 3 lbs. of water in twenty-four hours. A corn plant, in about three and a half months, gave off in vapour thirty-six times its own weight of water. A medium-sized forest tree will pump up and exhale about 5 barrels of water in twenty-four hours: this will give about 800 barrels to the acre. An acre of grain or grass will do about the same. From this it may be seen why forests exert such a powerful influence on the rainfall of a country.

Again, we may consider *the leaf as a lightning conductor*. It is one of the most efficient conductors of electricity ever made. Most leaves have notched edges; each of these "points" is powerful to attract the electric fluid from the air, and through the stem convey it silently to the ground. A single blade of grass is said to be three times as powerful to attract electricity as a fine cambric needle, and a twig covered with leaves is more efficient than the best constructed "patent point." A tree covered with leaves is the most efficient safeguard from lightning that can be found.

A green tree is constantly conveying electricity from the earth to the air and from the air to the earth. True, it sometimes tries to carry too large a load in response to the efficient collecting power of the leaves. They gather it in faster than the trunk can carry it away, and it is burst. We say the tree is struck with lightning; but it has often been struck before, but this time it was overloaded and crushed. Trees are natural lightning-rods, more efficient than all the artificial ones that have ever been invented.

In the next place we may contemplate *the leaf as an organiser* of organic matter. It is here that it has performed its most efficient and important service for man. Through its agency every particle of both vegetable and animal organism has been either directly or indirectly built up. Every plant, tree, and shrub has been directly built up through the labour of the leaf. And even long before the present order of things existed the leaf was at work: through its labours vast beds of vegetable matter were laid away far back in the carboniferous ages, which by heat and pressure have become coal, forming vast storehouses of excellent fuel. And still farther back, in times when Silurian seas washed the shores of limited bodies of land, the leaf was at earnest, ceaseless toil. Thus we owe to the leaf not only what makes life pleasant, but our food and raiment and fuel, without which life would be impossible. Without the leaf as an organiser the earth would sink back into a lifeless, pulseless waste.

Lastly, we may consider *the leaf as a chemical agent,* withdrawing and consolidating various poisonous gases, which if left in the air would render it unfit to sustain life, and thus convert the earth into one vast charnel-house of the dead. The air contains 1-2500th of its own bulk of carbonic acid, consisting of two equivalents of oxygen and one of carbon. This gas is a deadly foe to animal life, and if permitted to accumulate in the air would soon render it unfit to sustain life. And yet there are certain processes constantly going on that tend to augment the proportion of this gas in the atmosphere. Every breath of every human being and every living animal, and every bit of fuel that is consumed, and every particle of vegetable matter that decays, and every volcano that sends forth its deadly fumes, are adding to the quantity of this gas in the atmosphere. By what agency, then, is the equilibrium maintained? It is through the agency of our little friend the leaf that the work so essential to life and health is performed. It is constantly employed as an analytic chemist imbibing this poisonous gas and analysing it, using the carbon to build up the organic substance of its own structure, and giving up the healthful, life-giving oxygen to the atmosphere again. This process is so regulated as exactly to keep pace with the liberation of carbonic acid through the agencies mentioned above. Other deleterious gases are thus taken in and rendered innocuous. The blue gum (*Eucalyptus globulus*), of Australia, has become famous for absorbing the deadly gases in miasmatic districts, and thus rendering them healthy. Thus the leaf labours preparing food for all living animals, and raiment and fuel for the lords of creation, as well as all wood and bone and ivory used in the arts. It also purifies the air, making animal life possible, and clothing the earth with beauty that the life thus preserved may be replete with the highest enjoyment.—*Kansas City Review of Science and Industry.*

IV. MOLECULAR PHYSICS IN HIGH VACUA.*

By WILLIAM CROOKES, F.R.S.

WHEN I was asked, a month or two ago, to illustrate in this theatre some of my recent researches on Molecular Physics in High Vacua, I exclaimed "How is it possible to bring such a subject worthily before a Royal Institution audience when none of the experiments can be seen more than three feet off?" If to-night I am fortunate enough to show all the experiments to those who are not far distant, and if I succeed in making most of them visible at the far end of the theatre, such a success will be entirely due to the great kindness of your late Secretary, Mr. Spottiswoode, who has placed at my disposal his magnificent induction-coil,—not only for this lecture, but for some weeks past in my own Laboratory,—thus enabling me to prepare apparatus and vacuum tubes on a scale so large as to relieve me of all anxiety so far as the experimental illustrations are concerned.

Before describing the special researches in molecular physics which I propose to illustrate this evening, it is necessary to give a brief outline of one small department of the modern theory of the constitution of gases.

---

* A short-hand report of a Lecture delivered at the Royal Institution, on Friday, April 4, 1879.

It is not easy to make clear the kinetic theory, but I will try to simplify it in this way:—Imagine that I have in a large box a swarm of bees, each bee independent of its fellow, flying about in all manner of directions and with very different velocities. The bees are so crowded that they can only fly a very short distance without coming into contact with one another or with the sides of the box. As they are constantly in collision, so they rebound from each other with altered velocities and in different directions, and when these collisions take place against the sides of the box pressure is produced. If I take some of the bees out of the box, the distance which each individual bee will be able to fly before it comes into contact with its neighbour will be greater than when the box was full of bees, and if I remove a great many of the bees I increase to a considerable extent the average distance that each can fly without a collision. This distance I will call the bee's *mean free path*. When the bees are numerous the mean free path is very short; when the bees are few the mean free path will be longer, the length being inversely proportional to the number of bees present. Let us now imagine a loose diaphragm to be introduced in the centre of the box, so as to divide the number of bees equally. The same number of bees being on each side, the impacts on the diaphragm will be equal; and the mean speed of the bees being the same, the pressure will be identical on each side of the diaphragm, and it will not move.

Let me now warm one side of this division so as to let it communicate extra energy to a bee when it touches it. As before, a bee will strike the diaphragm with its normal mean velocity, but will be driven back with extra velocity, the reaction producing an increase of pressure on the diaphragm. It will be found, however, that although the diaphragm is free to move, the extra strength of the recoil on the warm side does not produce any motion. This at first sight seems contrary to the law of action and reaction being equal. The explanation is not difficult to understand. The bees which fly away from the diaphragm have drawn energy from it, and therefore move quicker than those which are coming towards it; they beat back the crowd to a greater distance, and keep a greater number from striking the diaphragm. Near to the heated side of the diaphragm the density is less than the average, while beyond the free path the density is above the average, and this greater crowding extends to all other parts of the box.

Thus it happens that the extra energy of the impacts against the warm side of the diaphragm is exactly compensated by the increased number of impacts on the cool side. In spite therefore of the increased activity communicated to a portion of the bees, the pressure on the two sides of the diaphragm will remain the same. This represents what occurs when the extent of the box containing the bees is so great, compared with the mean free path, that the abrupt change in the velocities of those bees which rebound from the walls of the box produces only an insensible influence on the motions of bees at so great a distance as the diaphragm.

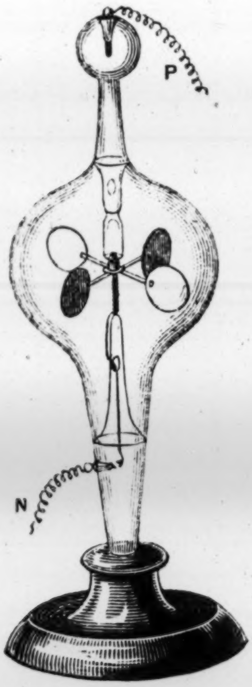
I will next ask you to imagine that I am gradually removing bees from our box, still keeping the diaphragm warm on one side. The bees getting fewer the collisions will become less frequent, and the distance each bee can fly before striking its neighbour will get longer and longer, and the crowding in front of them will grow less and less. The compensation will also diminish, and the warmed side of the diaphragm will have a tendency to be beaten back. A point will at last be reached on the warm side, when the mean free path of the bees will be long enough to admit of their dashing right across from the diaphragm to the side of the box, without meeting more than a certain number of in-coming bees in their flight. In this case the bees will no longer fly quite in the same direction as before. They will now fly less sideways, and more forwards and backwards between the heated face of the diaphragm and the opposed wall of the box. Because of this preponderating motion, and also because they will thereby less effectually keep back bees crowding in from the sides, there will now be a greater proportionate pressure both on the hot face of the diaphragm and on that part of the box which is in front of it. Hence the pressure on the hot side will now exceed that on the cool side of the diaphragm, which will consequently have a backward movement communicated to it.

I may diminish the size of the bees as much as I like, and by correspondingly increasing their number the mean free path will remain the same. Instead of bees let me call them molecules, and instead of having a few hundreds or thousands in the box let me have millions and billions and trillions; and if we also diminish the mean free path to a considerable extent, we get a rough outline of the kinetic theory of gases. (I may just mention that the mean free path of the molecules in air, at the ordinary pressure, is the ten-thousandth of a millimetre.)

Three years ago I had the honour of bringing before you the results of some researches on the Radiometer. Let me now take up the subject where I then left off. I have here two radiometers which have been rotating before you under the influence of a strong light shining upon them.

The explanation of the movement of the radiometer is this,—the light, or the total bundle of rays included in the term "light," falling upon the blackened side of the vanes, becomes absorbed, and thereby raises the temperature of the black side: this causes extra excitement of the air molecules which come in contact with it, and pressure is produced, causing the fly of the radiometer to turn round.

FIG. I.



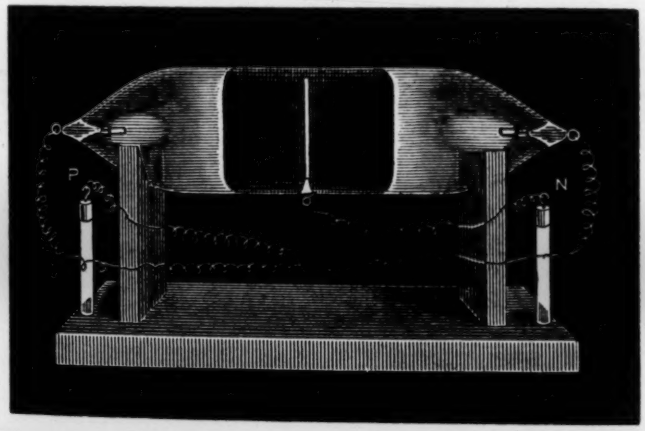
I have long believed that a well-known appearance observed in vacuum tubes is closely related to the phenomena of the mean free path of the molecules. When the negative pole is examined while the discharge from an induction-coil is passing through an exhausted tube, a dark space is seen to surround it. This dark space is found to increase and diminish as the vacuum is varied, in the same way that the ideal layer of molecular pressure in the radiometer increases and diminishes. As the one is perceived by the mind's eye to get greater, so the other is seen by the bodily eye to increase in size. If the vacuum is insufficient to permit the radiometer to turn, the passage of electricity shows that the "dark space" has shrunk to small dimensions. It is a natural inference that the dark space is the mean free path of the molecules of the residual gas.

The radiometer which has just been turning under the influence of the lime-light is not of the ordinary kind. Fig. 1 will explain its construction.

It is similar to an ordinary radiometer with aluminium disks for vanes, each disk coated on one side with a film of mica. The fly is supported by a hard steel instead of glass cup, and the needle point on which it works is connected by means of a wire with a platinum terminal sealed into the glass. At the top of the radiometer bulb a second terminal is sealed in. The radiometer can therefore be connected with an induction-coil, the movable fly being made the negative pole.

As soon as the pressure is reduced to a few millims. of mercury, a halo of velvety violet light forms on the metallic side of the vanes, the mica side remaining dark. As the pressure diminishes, a dark space is seen to separate the violet halo from the metal. At a pressure of half a millim. this dark space extends to the glass, and positive rotation commences. On continuing the exhaustion the dark space further widens out and appears to flatten itself against the glass, when the rotation becomes very rapid.

FIG. 2.



You perceive a dark space behind each vane and moving round with it. In the first experiment, radiation from the lime-light falling on the metallic sides of the vanes, produced a layer of molecular pressure which drove the fly round; so here the induction-current has produced molecular excitement at the surface of the vanes forming the negative pole, extending up to the side of the glass.

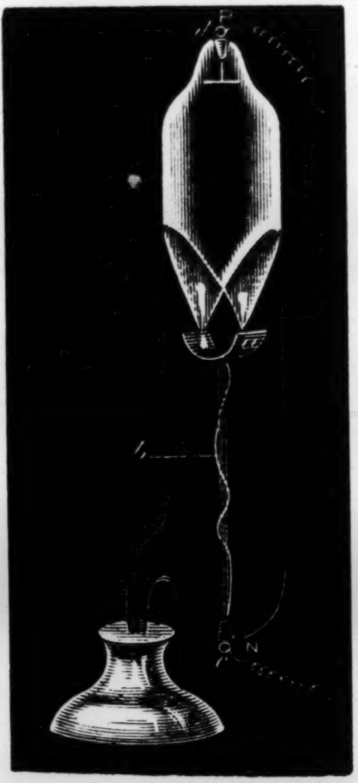
When the negative pole is in rapid rotation it is not easy to see this dark space, so I have arranged a tube in which the dark space will be visible to all present. The tube, as you will see by the diagram (Fig. 2), has a pole in the centre in the form of a metal disk, and other poles at each end. The centre pole is made negative, and the two end poles connected together are made the positive terminal. The dark space will be in the centre. When the exhaustion is not very great the dark space extends only a little distance on each side of the negative pole in the centre. When the exhaustion is very good, as it is in the tube before you, and I turn on the coil, the dark space is seen to extend for about 2 inches on each side of the pole.

Here, then, we see the induction spark actually illuminating the lines of molecular pressure caused by the excitement of the negative pole. The thickness of this dark space—nearly 2 inches—is the measure of the mean free path between successive collisions of the molecules of the residual gas. The extra velocity with which the negatively electrified molecules rebound from the excited pole keeps back the more slowly moving molecules which are advancing towards that pole. The conflict occurs at the boundary of the dark space, where the luminous margin bears witness to the energy of the discharge.

I will endeavour to throw on the screen an illustration of this dark space. A stream of water falls from a small jet on to a horizontal plate of glass. The water spreads over the plate and forms a thin film. The jet of water in the centre, from the velocity of its fall, drives the film of water before it on all sides, raising it into a ring-shaped heap. As I diminish the force of the jet the ring contracts: this is equivalent to the exhaustion getting less. When I increase the force of water the ring expands in size, the effect being analogous to an increase of exhaustion in my tubes. The extra velocity of the falling particles of water drive the in-coming particles of water before them, and raises a ridge round the side which exactly represents the luminous halo to the dark space to be seen in this tube.

If, instead of a flat disk, a metal cup is used for the negative pole, the successive appearances on exhausting the tube are somewhat different. The velvety violet halo forms over each side of the cup. On increasing the exhaustion the dark space widens out, retaining almost exactly the shape of the cup. The bright margin of the dark space becomes concentrated at the concave side of the cup to a luminous focus, and widens out at the convex side. When the dark space is very much larger than the cup, its outline forms an irregular ellipsoid drawn in towards the focal point. Inside the luminous boundary a dark violet light can be seen converging to a focus, and, as the rays diverge on the other side of the focus, spreading beyond the margin of the dark space; the whole appearance being strikingly similar to the rays of the sun reflected from a concave mirror through a foggy atmosphere. This proves a somewhat important point; it shows that the molecules thrown off the excited negative pole leave it in a direction almost normal to the surface.

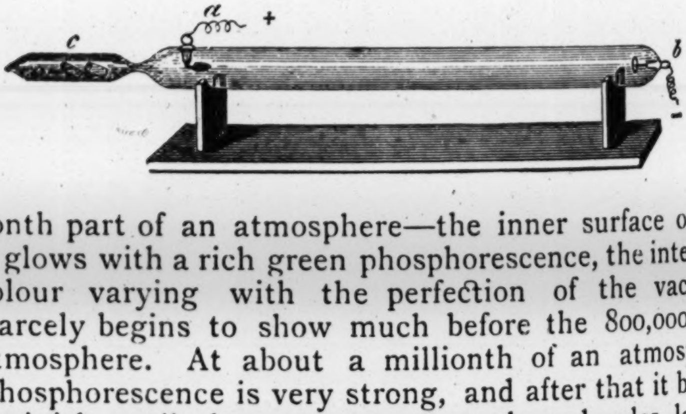
Fig. 3.



I can illustrate this property of the molecular rays by an experiment. This diagram (Fig. 3) is a representation of the tube which is before you. It contains, as a negative pole, a hemi-cylinder (*a*) of polished aluminium. This is connected with a fine copper wire, *b*, ending at the platinum terminal, *c*. At the upper end of the tube is another terminal, *d*. The induction-coil is connected so that the hemi-cylinder is negative and the upper pole positive, and when exhausted to a sufficient extent, as is the case with this tube, the projection of the molecular rays to a focus is very beautifully shown. The rays are driven from the hemi-cylinder in a direction normal to its surface; they come to a focus and then diverge, tracing their path in brilliant green phosphorescence on the surface of the glass.

You will notice that the rays which project from the negative pole and cross in the centre have a bright green appearance; that colour is entirely due to the phosphorescence of the glass. At a very high exhaustion the phenomena noticed in ordinary vacuum tubes when the induction spark passes through them—an appearance of cloudy luminosity and of stratifications—disappears entirely. No cloud or fog whatever is seen in the body of the tube, and with such a vacuum as I am working with in these experiments—about a millionth part of an atmosphere—the inner surface of the glass glows with a rich green phosphorescence, the intensity of colour varying with the perfection of the vacuum. It scarcely begins to show much before the 800,000th of an atmosphere. At about a millionth of an atmosphere the phosphorescence is very strong, and after that it begins to diminish until there are not enough molecules left to allow the spark to pass.*

FIG. 4.



I have here a tube which will serve to illustrate the dependence of the green phosphorescence of the glass on the degree of perfection of the vacuum (Fig. 4). The two poles are at *a* and *b*, and at the end (*c*) is a small supplementary tube connected with the other by a narrow aperture, and containing solid caustic potash. The tube has been exhausted to a very high point, and the potash heated so as to drive off moisture and deteriorate the vacuum.

---

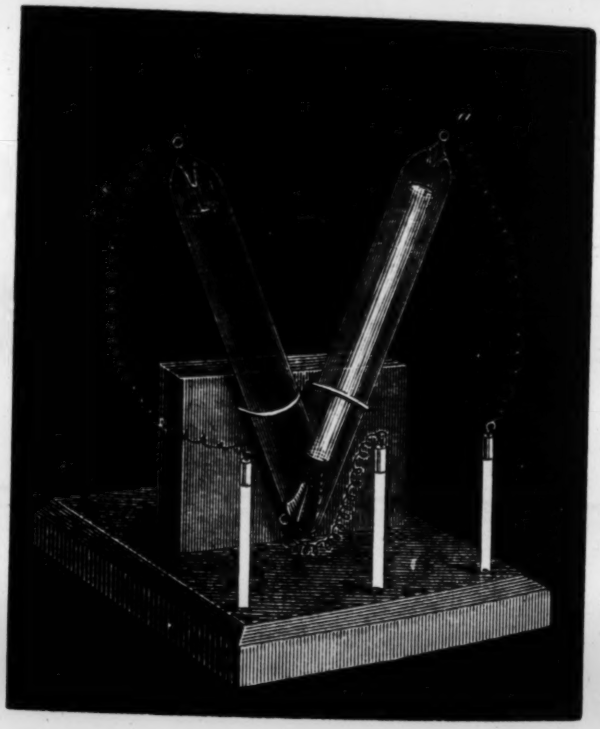
* 1·0 millionth of an atmosphere = 0·00076 millim.
1315·789 millionths of an atmosphere = 1·0 millim.
1,000,000 " " " = 760·0 millims.
" " " " = 1 atmosphere.

Exhaustion has then been re-commenced, and the alternate heating and exhaustion have been repeated until the tube has been brought to the state in which it now appears before you. When the induction spark is first turned on nothing is visible—the vacuum is so high that the tube is non-conducting. I now warm the potash slightly, and liberate a trace of aqueous vapour. Instantly conduction commences, and the green phosphorescence flashes out along the length of the tube. I continue the heat, so as to drive off more gas from the potash. The green gets fainter, and now a wave of cloudy luminosity sweeps over the tube, and stratifications appear. These rapidly get narrower, until the spark passes along the tube in the form of a narrow purple line. I take the lamp away, and allow the potash to cool; as it cools, the aqueous vapour, which the heat had driven off, is re-absorbed. The purple line broadens out, and breaks up into fine stratifications; these get wider, and travel towards the potash tube. Now a wave of green light appears on the glass at the other end, sweeping on and driving the last pale stratification into the potash; and now the tube glows over its whole length with the green phosphorescence. Would time allow I might keep it before you, and show the green growing fainter and the vacuum becoming non-conducting; but time is required for the absorption of the last traces of vapour by the potash, and I must pass on to the next subject.

This green phosphorescence is a subject that has much occupied my thoughts, and I have striven to ascertain some of the laws governing its occurrence. I soon perceived that the phosphorescence was not in the body of the tube itself, but was entirely on the surface of the glass. Another peculiarity of the rays producing this green phosphorescence is that they will not turn a corner in the slightest degree. Here is a V-shaped tube (Fig. 5), a pole being at each extremity. The pole at the right side (*a*) being negative, you see that the whole of the right arm is flooded with green light, but at the bottom it stops sharply, and will not turn the corner to get into the left side. When I reverse the current, and make the left pole negative, the green changes to the left side, always following the negative pole, leaving the positive side with scarcely any luminosity.

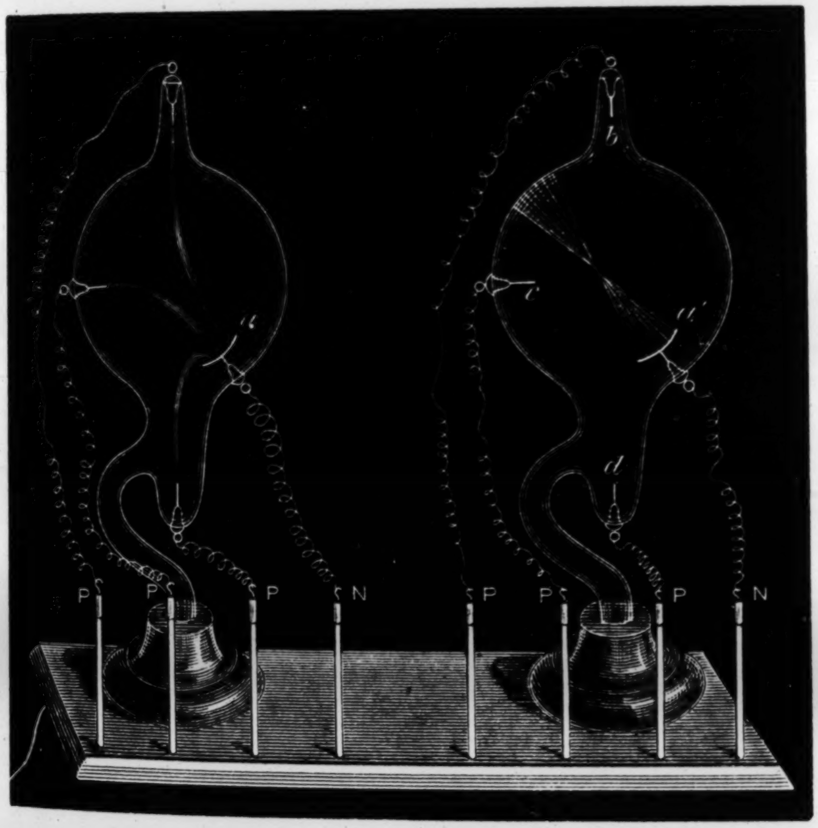
In the ordinary phenomena exhibited by vacuum tubes—phenomena with which we are all familiar—it is customary, for the more striking illustration of their contrasts of colour, to have the tubes bent into very elaborate designs. The positive luminosity caused by the phosphorescence of the residual gas follows all the convolutions and designs into which skilful glass-blowers can manage to twist the glass. The negative pole being at one end and the positive pole at the other, the luminous phenomena seem to depend more on the positive than on the negative at an ordinary exhaustion such as has hitherto been used to get the best phenomena of vacuum tubes. I have here two bulbs (Fig. 6), alike in shape and position of poles, the only difference being that one is at an exhaustion equal to a few millimetres of mercury,—such a moderate exhaustion as will give stratifications or the ordinary luminous phenomena,—whilst the other is exhausted to about the millionth of an atmosphere. I will first connect the moderately exhausted bulb with the induction-coil, and, retaining the pole at one side (*a*) always negative, I will put the positive wire successively to the other three poles with which the bulb is furnished. You will see that as I change the position of the positive pole, the line of violet light joining the two poles changes. In this moderately exhausted bulb, therefore, the electric current always chooses the shortest path between the two poles, and moves about the bulb as I alter the position of the wires.

FIG. 5.



This, then, is the kind of phenomenon we get in ordinary exhaustions. I will now try the same experiment with a tube that is highly exhausted, and, as before, will make the side pole (*a'*) the negative, the top pole (*b*) being positive. Notice how widely different is the appearance from that shown by the last bulb. The negative pole is in the form of a shallow cup. The bundle of rays from the cup crosses in the centre of the bulb, and thence diverging falls on the opposite side as a circular patch of green light. As I turn the bulb round you will all be able to see the faint blue focus and the green patch on the glass. Now observe, I remove the positive wire from the top, and connect it with the side pole (*c*). The green patch from the divergent negative focus is still there. I now make the lowest pole (*d*) positive, and the green patch still remains where it was at first, unchanged in position or intensity.

FIG. 6.

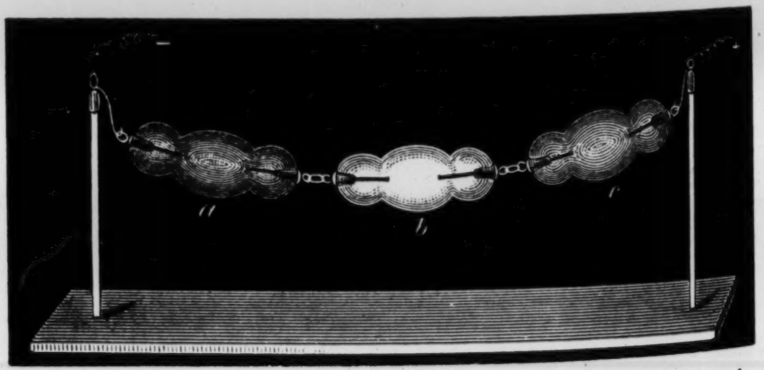


This, then, gives us another fact which brings us a little nearer to the cause of this green phosphorescence. It is this—

that in the low vacuum the position of the positive pole is of every importance, whilst in a high vacuum it scarcely matters at all where the positive pole is; the phenomena seem to depend entirely on the negative pole. In very high vacua, such as we have been using, the phenomena follow altogether the negative pole. If the negative pole points in the direction of the positive, all very well, but if the negative pole is entirely in the opposite direction it does not matter: the line of rays is projected all the same in a straight line from the negative.

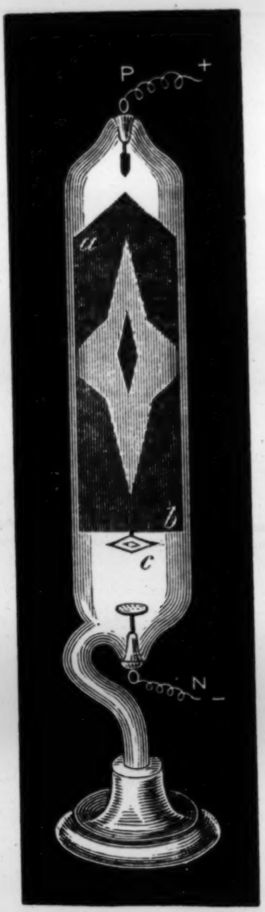
I have hitherto spoken of and illustrated these phenomena in connection with *green* phosphorescence. It does not follow, however, that the phosphorescence is always of that colour. This colouration is a property of the particular kind of glass in use in my laboratory. I have here (Fig. 7) three bulbs composed of different glass: one is uranium glass (*a*), which phosphoresces of a dark green colour; another is English glass (*b*), which phosphoresces of a blue colour; and the third (*c*) is soft German glass,—of which most of the apparatus before you is made,—which phosphoresces of a bright apple-green colour. It is therefore plain that this particular green phosphorescence is solely due to the glass which I am using. Were I to use English glass I should have to speak of blue phosphorescence, but I know of no glass which is equal to the German in brilliancy.

FIG. 7.



My earlier experiments were almost entirely carried on by the aid of the phosphorescence which glass takes up when it is under the influence of the electric discharge *in vacuo;* but many other substances possess this phosphorescent power, and some have it in a much higher degree than glass. For instance, here is some of the luminous sulphide of calcium prepared according to M. Ed. Becquerel's description. When it is exposed to light—even candlelight—it phosphoresces for hours with a rich blue colour. I have prepared a diagram with large letters written in this luminous sulphide; before it is exposed to the light the letters are invisible, but Mr. Gimingham has just exposed it in another room to burning magnesium, and now it is brought into the darkened theatre you will see the word "φως,"—*light*, a very suitable word for so beautiful a phosphorescence—shining brightly in luminous characters. The first letter, φ, shines with an orange light; it is a sulphide of calcium prepared from oyster-shells. The other letters, shining with a blue light, are sulphide of calcium prepared from precipitated carbonate of lime. Once the phosphorescence is excited the letters shine for several hours. I will put the diagram at the back, and we shall see how it lasts during the remainder of the lecture. This substance, then, is phosphorescent to light, but

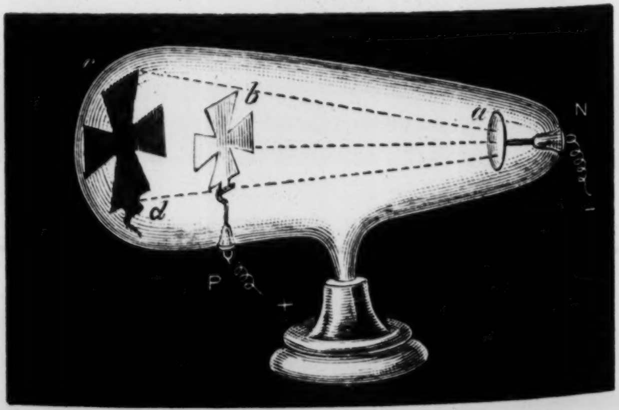
FIG. 8.



it is also much more strongly phosphorescent to the molecular discharge in a good vacuum, as you will see when I pass the discharge through this tube (Fig. 8). The white plate (*a*, *b*) in the centre of the tube is a sheet of mica painted over with the luminous sulphide of which the letter ϕ was composed in the diagram you have just seen. On connecting the poles with the coil, the mica screen glows with a strong yellowish green light, bright enough to illuminate all the apparatus near it. But there is another phenomenon to which I now desire to draw attention: on the luminous screen is a kind of distorted star-shaped figure. A little in front of the negative pole I have fixed a star (*c*) cut out in aluminium, and it is the image of this star which you see on the screen. It is evident that the rays coming from the negative pole project an image of anything that happens to be in front of it. The discharge, therefore, must come from the pole in straight lines, and does not merely permeate all parts of the tube and fill it with light as it would were the exhaustion less good. Where there is nothing in the way the rays strike the screen and produce phosphorescence, and where there is an obstacle they are obstructed by it, and a shadow is thrown on the screen. I shall have more to say about this shadow presently; I merely now wish to establish the fact that these rays driven from the negative pole produce a shadow.

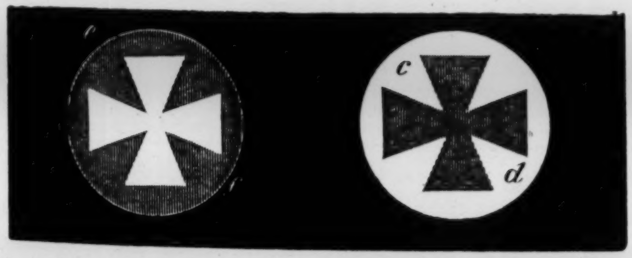
I must draw your attention to an important experiment connected with these molecular rays, but unfortunately it is a very delicate one, and very difficult to show to many at once; but I hope, if you know beforehand what to look for, you will all be able to see what I wish to show. In this pear-shaped bulb (Fig. 9 A) the negative pole (*a*) is at the pointed end. In the middle is a cross (*b*)

FIG. 9 A.



cut out of sheet aluminium, so that the rays from the negative pole projected along the tube will be partly intercepted by the aluminium cross, and will project an image of it on the hemispherical end of the tube which is phosphorescent. I think you will all now see the shadow of the cross on the end of the bulb (*c*, *d*), and notice that the cross is black on a luminous ground. Now, the rays from the negative pole have been passing by the side of the aluminium cross to produce the shadow; they have been hammering and bombarding the glass till it is appreciably warm, and at the same time they have been producing another effect on that glass—they have deadened its sensibility. The glass has got tired, if I may use the expression, by the enforced phosphorescence. Some change has been produced by this bombardment which will prevent the glass from responding easily to additional excitement; but the part that the shadow has fallen on is not tired—it has not been phosphorescing at all and is perfectly fresh; therefore if I throw this star down,—I can easily do so by giving the apparatus a slight jerk, for it has been most ingeniously constructed with a hinge by Mr. Gimingham,—and so allow the rays from the negative pole to fall uninterruptedly on to the end of the bulb, you will suddenly see the black cross (*c*, *d*, Fig. 9B) change to a luminous one (*e*, *f*), because the background is only faintly phosphorescing, whilst the part which had the black shadow on it retains its full phosphorescent power. The luminous cross is now dying out. This is a most delicate and venturous experiment, and I am fortunate in having succeeded so well, for it is one that cannot be rehearsed. After resting for a time the glass seems to partly recover its power of phosphorescing, but it is never so good as it was at first.

FIG. 9 B.



We have, therefore, found an important fact connected with this phosphorescence. Something is projected from the negative pole which has the power of hammering away at the glass in front of it, in such a way as to cause it not only to vibrate and become temporarily luminous while the discharge is going on, but to produce an impression upon the glass which is permanent. The explanation which has gradually evolved itself from this series of experiments is this:—The exhaustion in these tubes is so high that the dark space, as I showed you at the commencement of this Lecture, that extended around the negative pole, has widened out till it entirely fills the tube. By great rarefaction the mean free path has become so long that the hits in a given time may be disregarded in comparison to the misses, and the average molecule is now allowed to obey its own motions or laws without interference. The mean free path is in fact comparable to the dimensions of the vessel, and we have no longer to deal with a *continuous* portion of matter, as we should were the tubes less highly exhausted, but we must here contemplate the molecules *individually*. At first this was only a convenient working hypothesis. Long-continued experiment then raised this provisional hypothesis almost to the dignity of a theory, and now the general opinion is that this theory gives a fairly correct explanation of the facts. In these highly exhausted vessels the mean free path of the residual molecules of gas is so long that they are able to drive across from the pole to the other side of the tube with comparatively few collisions. The negatively electrified molecules of the gaseous residue in the tube therefore dash against anything that is in front, and cast shadows of obstacles just as if they were rays of light. Where they strike the glass they are stopped, and the production of light accompanies this sudden arrest of velocity.

Other substances besides English, German, and uranium glass, and Becquerel's luminous sulphides, are also phosphorescent. I think, without exception, the diamond is the most sensitive substance I have yet met for ready and brilliant phosphorescence. I have here a tube, similar to those already exhibited, containing a mica screen painted with powdered diamond, and when I turn on the coil, the brilliant blue phosphorescence of the diamond can be seen, quite overpowering the green phosphorescence of the glass. Here, again, is a very curious diamond, which I was fortunate enough to meet with a short time ago. By daylight it is green, produced, I fancy, by an internal fluorescence. The diamond is mounted in the centre of this exhausted bulb (Fig. 10), and the negative discharge will be directed on it from below upwards. On darkening the theatre you see the diamond shines with as much light as a candle, phosphorescing of a bright green.

In this other bulb is a remarkable collection of crystals of diamonds, which have been lent me by Professor Maskelyne. When I pass the discharge over them I am afraid you will only be able to see a few points of light, but if you will examine them after the Lecture, you will see them phosphoresce with a most brilliant series of colours—blue, apricot, red, yellowish green, orange, and pale green.

FIG. 10.

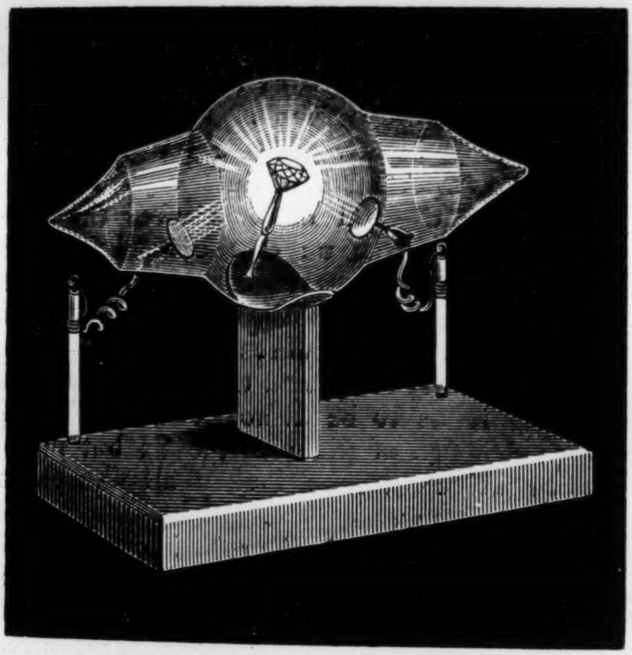
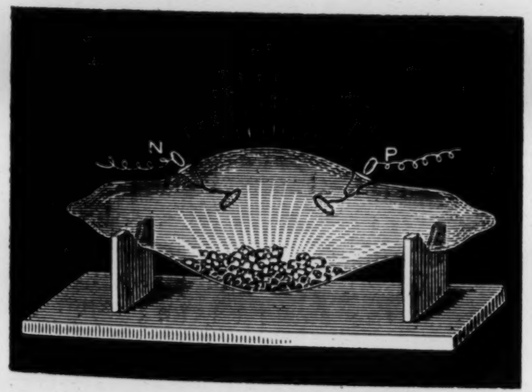


FIG. 11.



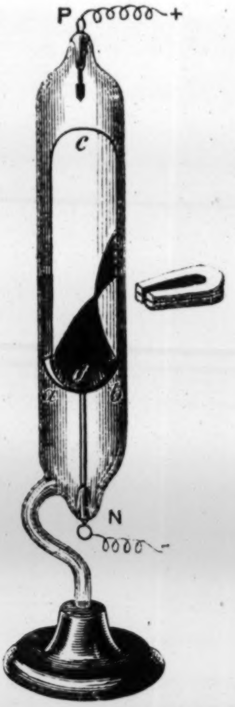
Next to the diamond the ruby is one of the most remarkable stones for phosphorescing. In this tube (Fig. 11) is a collection of ruby pebbles, for the loan of which I am indebted to my friend Mr. Blogg, of the firm of Blogg and Martin, who placed a small sackful at my disposal. As soon as I turn on the induction spark you will see these rubies shining with a brilliant rich red colour, as if they were glowing hot. Now the ruby is nothing but crystallised alumina with a little colouring-matter, and it became of great interest to ascertain whether the artificial ruby made by M. Feil, of Paris, would glow in the same manner. I had simply to make my wants known to M. Feil, and he immediately sent me a box containing artificial rubies and crystals of alumina of all sizes, and from those I have selected the mass in this tube which I now place under the discharge: they phosphoresce of the same rich red colour as the natural ruby. It scarcely matters what colour the ruby is, to begin with. In this tube of natural rubies there are stones of all colours—the deep red ruby and the pale pink ruby. There are some so pale as to be almost colourless, and some of the highly-prized tint of pigeon's blood; but in the vacuum under the negative discharge they all phosphoresce with about the same colour.

As I have just mentioned, the ruby is crystallised alumina. In a paper published twenty years ago by Ed. Becquerel[*] I find that he describes the appearance of alumina as glowing with a rich red colour in the phosphoroscope (an instrument by which the duration of phosphorescence in the sunlight can be examined). Here is some chemically pure precipitated alumina which I have prepared in the most careful manner. It has been heated to whiteness, and you see it glows with the rich red colour which is supposed to be characteristic of alumina. The mineral known as corundum is a colourless variety of crystallised alumina. Under the negative discharge in a vacuum, corundum phosphoresces of a rose-pink colour. There is another curious fact in which I think chemists will feel interested. The sapphire is also crystallised alumina, just the same as the ruby. The ruby has a little colouring-matter in it, giving it a red colour; the sapphire has a colouring-matter which gives it a blue colour, whilst corundum is white. I have here in a tube a very fine crystal of sapphire, and, when I pass the discharge over it, it gives alternate bands of red and green. The red we can easily identify with the glow of alumina; but what is the green? If alumina is precipitated and purified as carefully as in the case I have just mentioned, but in a somewhat different manner, it is found to glow with a rich green colour. Here are the two specimens of alumina in tubes, side by side. Chemists would say that there was no difference between one and the other; but I connect them with the induction-coil, and you see that one glows with a bright green colour, whilst the other glows with a rich red colour. Here is a fine specimen of chemically pure alumina, lent me by Messrs. Hopkin and Williams; by ordinary light it is a perfectly white powder. It is just possible that the rich fire of the ruby, which has caused it to be so prized, may be due not entirely to the colouring-matter, but to its wonderful power of phosphorescing with a deep red colour, not only under the electric discharge in a vacuum, but whenever exposed to a strong light.

---

[*] Annales de Chimie et de Physique, 3rd series, vol. lvii., p. 50, 1859.

Fig. 12.



The spectrum of the red light emitted by all these varieties of alumina—the ruby, corundum, or artificially precipitated alumina—is the same as described by Becquerel twenty years ago. There is one intense red line, a little below the fixed line B in the spectrum, having a wavelength of about 6895. There is a continuous spectrum beginning at about B, and a few fainter lines beyond it, but they are so faint in comparison with this red line that they may be neglected. This line may be called the characteristic line of alumina.

I now pass on to another fact connected with this negative discharge. Here is a tube (Fig. 12) with a negative pole (*a*, *b*) in the form of a hemi-cylinder, similar to the one you have already seen (Fig. 3), but in this case I receive the rays on a phosphorescent screen (*c*, *d*). See how brilliantly the lines of discharge shine out, and how intensely the focal point is illuminated; it lights the whole table. Now I bring a small magnet near, and move it to and fro; the rays obey the magnetic force, and the focus bends one way and the other as the magnet passes it. I can show this magnetic action a little more definitely. Here is a long glass tube (Fig. 13), very highly exhausted, with a negative pole at one end (*a*) and a long phosphorescent screen (*b*, *c*) down the centre of the tube. In front of the negative pole is a plate of mica (*b*, *d*) with a hole (*e*) in it, and the result is that when I turn on the current, a line of phosphorescent light (*e*, *f*) is projected along the whole length of the tube. I now place beneath the tube a powerful horseshoe magnet: see how the line of light becomes curved under the magnetic influence (*e*, *g*), waving

FIG. 13.

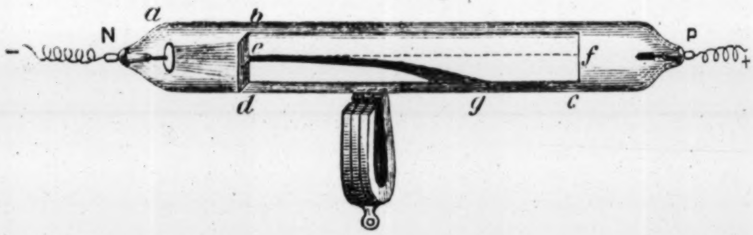
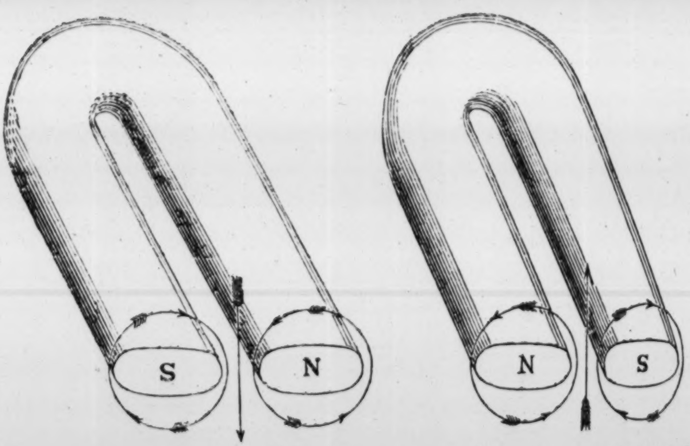


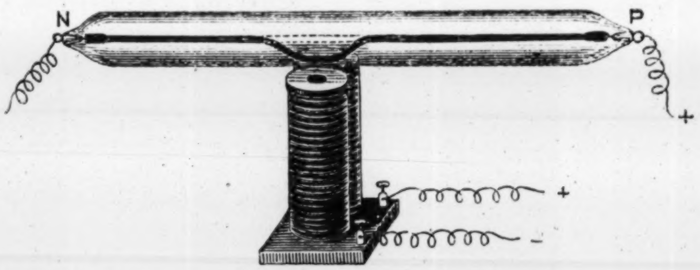
FIG. 14.



about like a flexible wand as I move the magnet up and down. The action of the magnet can be understood by reference to this diagram (Fig. 14). The north pole gives the ray of molecules a spiral twist one way, and the south pole twists it the other way; the two poles side by side compel the ray to move in a straight line up or down, along a plane at right angles to the plane of the magnet and a line joining its poles.

Now it is of great interest to ascertain whether the law governing the magnetic deflection of the trajectory of the molecules is the same as has been found to hold good at a lower vacuum. The former experiment was with a very high vacuum. This is a tube with a low vacuum (Fig. 15).

FIG. 15.



On passing the induction spark it passes as a narrow line of violet light joining the two poles. Underneath I have a powerful electro-magnet. I make contact with the magnet, and the line of light dips in the centre towards the magnet. I reverse the poles, and the line is driven up to the top of the tube. Notice the difference between the two phenomena. Here the action is temporary. The dip takes place under the magnetic influence; the line of discharge then rises, and pursues its path to the positive pole. In the high exhaustion, however, after the ray of light had dipped to the magnet it did not recover itself, but continued its path in the altered direction.

During these experiments another property of this molecular discharge has made itself very evident, although I have not yet drawn attention to it. The glass gets very warm where the green phosphorescence is strongest. The molecular focus on the tube, which we have just seen (Fig. 12) would be intensely hot, and I have prepared an apparatus by which this heat at the focus can be intensified and rendered visible to all present. This small tube (*a*) (Fig. 16) is furnished with a negative pole in the form of a cup (*b*). The rays will therefore be projected to a focus in the middle of the tube (Fig. 17, *a*). At the side of the tube is a small electro-magnet, which I can set in action by touching a key, and the focus is then drawn to the side of the glass tube (Fig. 17, *b*). To show the

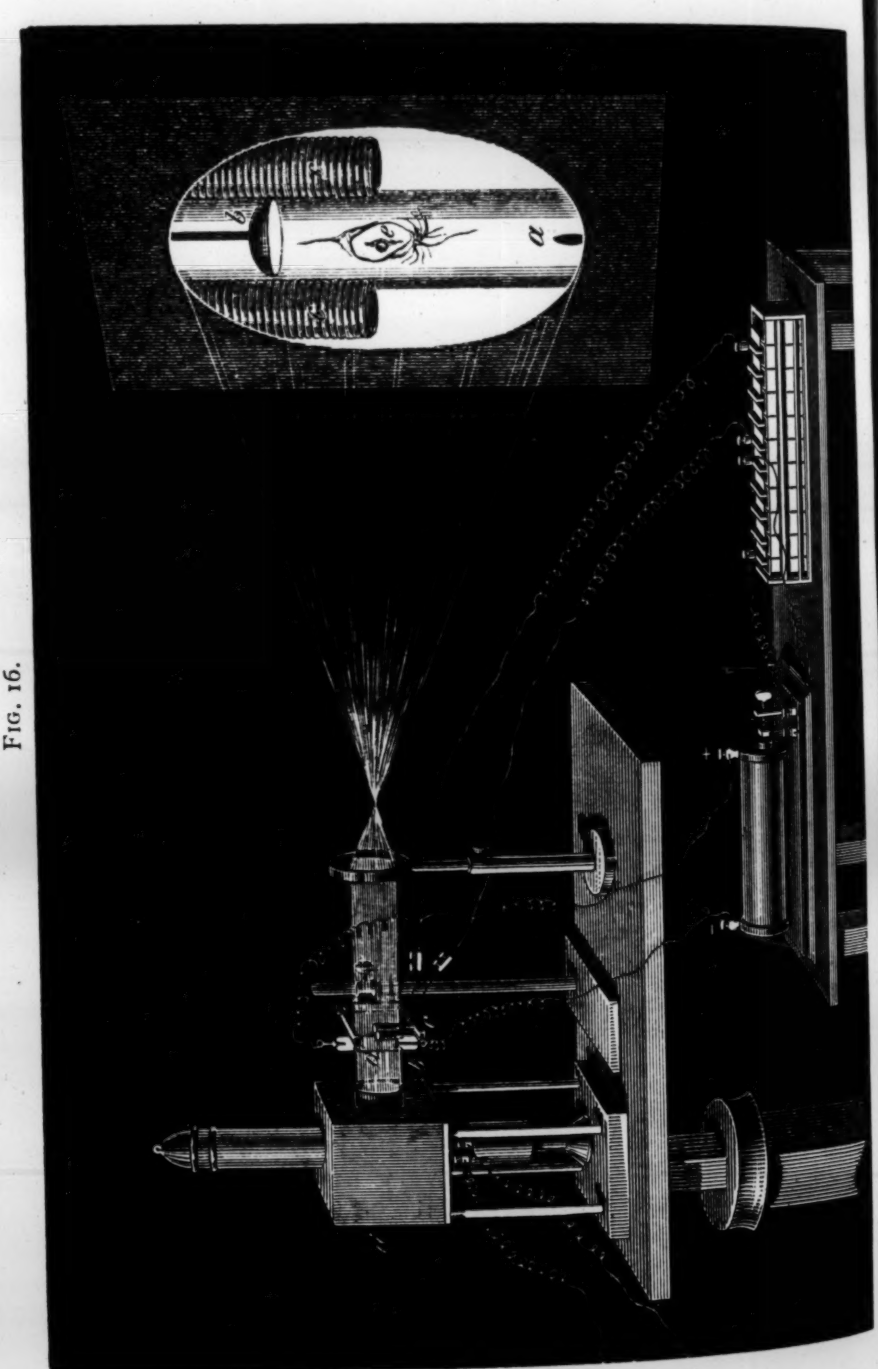
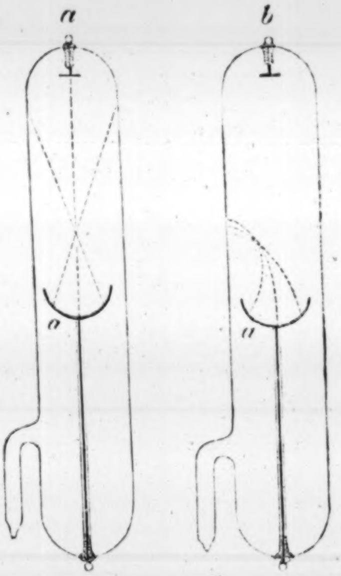


FIG. 16.

first action of the heat I have coated the tube with wax. I will put the apparatus in front of the electric lantern (*d*), and throw a magnified image of the tube on the screen. The coil is now at work, and the focus of

FIG. 17.

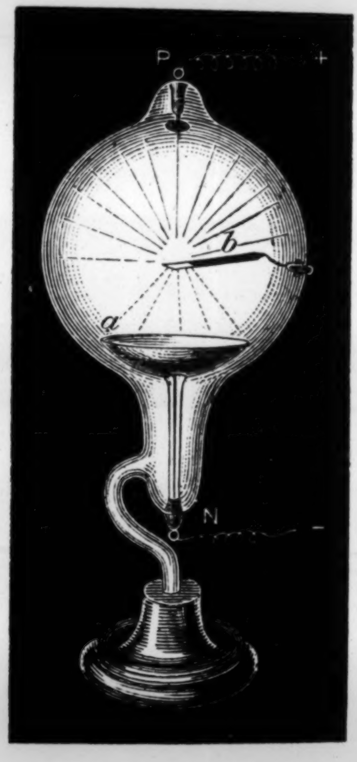


molecular rays is projected along the tube. I turn the magnetism on, and draw the focus on the side of the glass. The first thing you see is a small circular patch melted in the coating of wax. The glass soon begins to disintegrate, and cracks are shooting starwise from the centre of heat. The glass is softening. Now the atmospheric pressure forces it in, and now it melts. A hole (*e*) is perforated in the middle, the air rushes in, and the experiment is at an end.

Instead of drawing the focus to the side of the glass with a magnet, I will take another tube (Fig. 18), and allow the focus from the cup-shaped negative pole (*a*) to play on a piece of platinum wire (*b*) which is supported in the centre of the bulb. The platinum wire not only gets white-hot, but you can see sparks coming from it on all sides, showing that it is actually melting.

Here is another tube, but instead of platinum I have put in the focus that beautiful alloy of platinum and iridium which Mr. Matthey has brought to such perfection, and I think that I shall succeed in even melting that. I first turn on the induction-coil slightly, so as not to bring out its full power. The focus is now playing on the iridio-platinum, raising it to a white-heat. I bring a small magnet near, and you see I can deflect the focus of heat just as I did the luminous focus in the other tube. By shifting the magnet I can drive the focus up and down, or draw it completely away from the metal, and render it non-luminous.

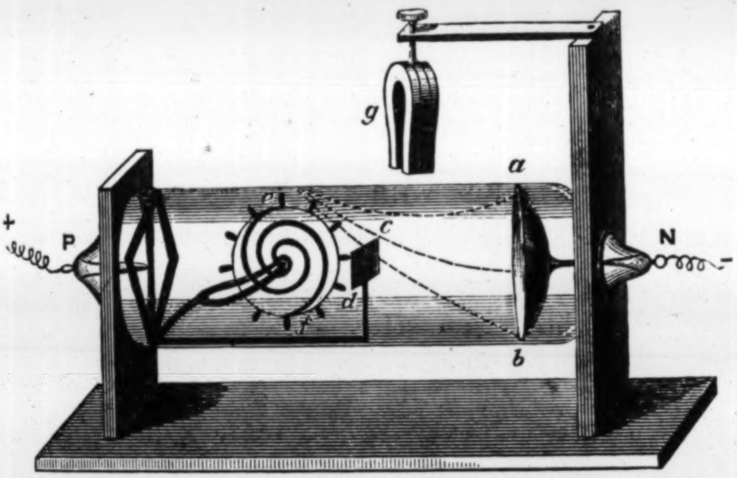
FIG. 18.



I withdraw the magnet, and let the molecules have full play again; the metal is now white-hot. I increase the intensity of the spark. The metal glows with almost insupportable brilliancy, and at last melts.

There is still another property of this molecular discharge, and it is this:—You have seen that the molecules are driven violently from the negative pole. If I place something in front of these molecules, they show the force of impact by the heat which is produced. Can I make this mechanical action evident in a more direct way? Nothing is simpler. I have only to put some easily moving object in the line of discharge in order to get a powerful mechanical action. Mr. Gimingham, with great skill, has constructed a piece of apparatus which I will presently put in the electric lantern, so that all will be able to see its action. But first I will explain the construction by means of this diagram (Fig. 19). The negative pole (*a*, *b*) is in the form of a very shallow cup. In front of the cup is a mica screen (*c*, *d*), wide enough to intercept nearly all the molecular rays coming from the negative pole. Behind this screen is a mica wheel (*e*, *f*) with a series of vanes, making a sort of paddle-wheel of it. So arranged, the molecular stream from the pole *a b* will nearly all be cut off from the wheel, and what escapes over and under the screen will hit the vanes equally, and will not produce any movement. I now put a magnet, *g*, over the tube, so as to deflect the stream over or under the obstacle *c d*, and the result will be rapid motion in one or the other direction, according to the way the magnet is turned. I now throw the image of the apparatus on the screen. The spiral lines painted on the wheel show which way it turns. I arrange the magnet to draw the molecular stream so as to beat against the upper vanes, and the wheel revolves rapidly, as if it were an over-shot water-wheel. I now turn the magnet so as to drive the molecular stream underneath; the wheel slackens speed, stops, and then begins to rotate the other way, as if it were an under-shot water-wheel. This can be repeated as often as I like to reverse the position of the magnet, the change of rotation of the wheel showing immediately the way the molecular stream is deflected.

FIG. 19.



This experiment illustrates the last of the phenomena which time allows me to bring before you, attending the passage of the induction spark through a highly exhausted atmosphere. It will now be naturally asked, What have we learned from the phenomena described and exhibited, and from the explanations that have been proposed? We find in these phenomena confirmation of the modern views of matter and energy. The facts elicited are in harmony with the theory that matter is not continuous but composed of a prodigious number of minute particles, not in mutual contact. The facts also are in full accordance with the kinetic theory of gases—to which I have already referred—and with the conception of heat as a particular kind of energy, expressing itself as a rapid vibratory motion of the particles of matter. This alone would be a lesson of no small value. In Science, every law, every generalisation, however well established, must constantly be submitted to the ordeal of a comparison with newly-discovered phenomena; and a theory may be pronounced triumphant when it is found to harmonise with and to account for facts which when it was propounded were still unrecognised or unexplained.

But the experiments have shown us more than this: we have been enabled to contemplate matter in a condition hitherto unknown,—in a fourth state,—as far removed from that of gas as gas is from liquid, where the well-known properties of gases and elastic fluids almost disappear, whilst in their stead are revealed attributes previously masked and unsuspected. In this ultra-gaseous state of matter phenomena are perceived which in the mere gaseous condition are as impossible as in liquids or solids.

I admit that between the gaseous and the ultra-gaseous state there can be traced no sharp boundary; the one merges imperceptibly into the other. It is true also that we cannot see or handle matter in this novel phase. Nor can human or any other kind of organic life conceivable to us penetrate into regions where such ultra-gaseous matter may be supposed to exist. Nevertheless, we are able to observe it and experiment on it, legitimately arguing from the seen to the unseen.

Of the practical applications that may arise out of these researches, it would be now premature to speak. It is rarely given to the discoverer of new facts and new laws to witness their immediate utilisation. The ancients showed a perhaps unconscious sagacity when they selected the olive, one of the slowest growing trees, as the symbol of Minerva, the goddess of Arts and Industry. Nevertheless, I hold that all careful honest research will ultimately even though in an indirect manner draw after it, as Bacon said, "whole troops of practical applications."

NOTICES OF BOOKS.

Transactions and Proceedings of the Royal Society of Victoria. Vols. xiii. and xiv. Melbourne: Mason, Firth, and McCutcheon. London: Williams and Norgate.

We cannot congratulate the Royal Society of Victoria on the subjects of the papers printed in the thirteenth volume of their "Transactions." More than half the volume is taken up with a treatise on practical geodesy, and of the remaining memoirs there is not one which might not quite as easily have been written in London, Paris, or Berlin. What the learned world tacitly expects from such societies is that they shall prominently, and before all things, busy themselves with the rich crop of unrecorded facts and phenomena which surrounds them, waiting, so to speak, for recognition. The geology, the palæontology, botany, zoology, and ethnology of their own region constitute the sphere in which they can render the greatest services to Science and win for themselves the highest honour.

Vol. xiv. contains four noteworthy papers:—"Notes on the Coast-line of the Western District, and Proofs of the Uniform Condition of Meteorological Phenomena over Long Periods of Time," by Mr. T. E. Rawlinson; on some "New Marine Mollusca," by the Rev. J. E. Tennison-Woods—a most able and persevering scientific worker; "Extracts from a Diary in Japan," by F. C. Christy; and "History of Palæozoic Actinology in Australia," by R. Etheridge, jun. The last-mentioned memoir is a summary of what has been already discovered concerning the fossil corals of Australia. Mr. Christy's diary teems with interesting facts concerning the climate, the vegetation, agriculture, and horticulture, the architecture, religion, government, domestic economy, mining, and natural history of these wonderful islands. The author remarks that in Japan eight adults live from the produce of one acre, and keep it in good condition with their excreta, whilst in England the excreta from 800 to 1200 persons are used per acre without profitable result. The fauna of Japan agrees wonderfully closely with that of Europe. Mr. Christy gives a list—too long for insertion here—of butterflies, moths, and bees common to Japan and England, and remarks:—"In referring to the very many species identical with those of England it is remarkable, because Japan consists of a series of islands so very distant and isolated from England, and goes far to disprove Darwin's theory that the farther species are from species—that is, the more they are diffused by distance—the more they must differ, having to struggle for existence over so great a space." It must, however, be remembered that the isolation between Britain and Japan is less in reality than in seeming. Both are merely outlying portions of one and the same zoological region with which they were at one time actually connected. Most of the species enumerated by Mr. Christy are traceable across the European and Asiatic continents.

Victorian Year-Book for 1877-8. By H. H. HAYTER, Government Statist of Victoria. Melbourne: J. Ferries and G. Robertson. London: G. Robertson.

FROM among the valuable matter to be found in it we extract the following account of the appliances for scientific education existing in the colony of Victoria:—There are Schools of Mines both at Ballarat and Sandhurst. At the former there are classes for different branches of mathematics, for engineering, surveying both above and below ground, chemistry, metallurgy and assaying, telegraphy, and the French and German languages. The chemical laboratory contains twenty work-tables for students, and in the metallurgical laboratory there are twelve reducing furnaces. During 1877 the number of students in the January, April, July, and October terms respectively was 48, 58, 79, and 60. The Museum of the Institution contains 1594 mineralogical and geological specimens, besides models, but—like the library of three hundred bound books—is obviously still in its infancy. The buildings of the Melbourne Public Library have cost £111,604, and are still unfinished. The total number of volumes at the end of 1877 was 101,276.

The Industrial and Technological Museum contains 1100 publications, 23,725 specimens, and 112 drawings. Class lectures are given here on chemistry, mining, and telegraphy, the last-mentioned subject attracting as many pupils as the two former conjointly.

The so-called National Museum occupies a building on the grounds of Melbourne University. It comprises specimens of minerals, stuffed animals and birds, insects, "and other objects of curiosity,"—an expression which savours of the pre-scientific ages when biological collections were not understood to be means for study.

The account of Melbourne University throws no light on the all-important question whether it is preparing students for actual research or merely turning out examinees.

une,

t the
an in
l the
ually
y are

vern-
nd G.

tract
ation
ines
sses
ying
say-
The
nts,
ing
ary,
and
cal
ary
cy.
ost
es

b.
es
t-
er

e
f
s

Journal of Proceedings of the Royal Society of New South Wales for 1877. Vol. xi. Sydney: Richards. London: Trübner and Co.

We may fairly congratulate the Royal Society of New South Wales both on the quantity and quality of the work it is doing. Of the nineteen papers herein contained all save three deal—and deal usefully—with local facts. We must more particularly notice an account of *Dromornis Australis*, a newly-discovered extinct gigantic bird of Australia. The species was founded on a femur discovered at Peak Downs, in Queensland, which the Rev. W. B. Clarke and M. G. Krefft at first referred to the genus *Dinornis*, and which consequently seemed a proof of a whilom land-connection between New Zealand and Australia. Prof. Owen, however, pointed out that this bone agrees in its essential characters more with the emu than the moa, and that it indicates the former existence of a bird nearly of the stature of the ostrich, but with relatively shorter and stronger hind limbs. Hence it appears that Australia, like all other tracts of land in the southern hemisphere, whether insular or continental, was at one time inhabited by gigantic "flightless" birds.

The Rev. W. B. Clarke communicates another paper on a new fossil extinct kangaroo (*Sthenurus minor*), part of the skull of which was found in the shaft of a gold-lead in Phillip County, New South Wales. He expresses a fear that in the indiscriminate slaughter of marsupials now in progress some species not yet recognised may perish unnoticed.

A paper on the "Forest Vegetation of Central and Northern New England" may at first sight be open to a misunderstanding. This name has, as if for the very purpose of creating confusion, been given to a district in Australia. The author, Mr. W. Christy, gives an account of a carnivorous plant *Drosera peltata*, —the "bottle-weed" of the squatters,—which is accused of causing disease in sheep, There is also a description of twenty-one species of *Eucalyptus* growing in this region.

Dr. W. J. Barkas communicates memoirs on the "Sphenoid, Cranial Bones, Operculum, and supposed Ear Bones of *Ctenodus*," and on a "Dental Peculiarity of the Lepidosteidæ."

Natural History of Victoria. Prodromus of the Zoology of Victoria, or Figures and Descriptions of the Living Species of all Classes of the Victorian Indigenous Animals. Decade I. By Frederick McCoy. Melbourne: J. Ferries and G. Robertson. London: Trübner and Co.

We have here the beginning of what promises to be a most valuable work, creditable alike to its author and the Victorian government. The animal species are carefully figured in their natural colours, and the descriptions of their characteristics, general structure, habits, and locality are full and apparently accurate. The first three plates are devoted to three of the most dangerous snakes of the province—the black snake (*Pseudechys porphyriacus*), the copper-head (*Hoplocephalus superbus*), and the tiger-snake (*H. curtus*). Concerning these species there prevails some confusion of nomenclature, which tends to render accounts of the effect of their bite and of the value of antidotes untrustworthy. Thus in Tasmania the tiger-snake is known as "carpet-snake," a name given on the mainland to an innocent species. In the same island the black snake has received the name of "diamond-snake," which properly belongs to a harmless species of New South Wales. As regards the bites of these Australian "death-snakes," ammonia taken internally or injected into a vein has in some cases at least proved successful, whilst for the bite of the Indian cobra it appears to be invariably useless. This important distinction suggests some questions weighty at once from a speculative and from a practical point of view. Do the poisons of different serpents differ merely in concentration, or are they chemically distinct, requiring consequently a distinct line of treatment? If so, do we find identity, or at least close relation, among the venoms derived from snakes of the same family, just as in the vegetable kingdom we find allied groups of alkaloids pervading certain families, as in the Cinchonaceæ, the Strychnaceæ, &c. The chemists, and no less the biologists, of Australia have here a most enviable field for research laid before them.

Plate VII. is devoted to *Megascolides Australis*, the giant earthworm, which, when fully extended, is about 6 feet long. The remaining three plates represent Lepidopterous insects. One of these, *Agarista glycine*, a day-flying moth of the curious family Alraniidæ, is remarkable for the change of habits it has undergone. Its original food was the common weed *Gnaphalium luteo-album*. But since the planting of vineyards it has completely abandoned its former food, and devours the leaves of the vine, occasioning enormous injury. How the female moth could learn that a foreign plant having no structural resemblance to the *Gnaphalium* would yet afford suitable nourishment to her future brood is one of the many "nuces zoologicæ" which our successors may some day crack. Certainly she could not in this case be guided by "hereditary habit." Fowls will not eat them, and the Indian minah—introduced into the colony as a vermin-killer—has developed a taste for grapes instead of devouring the larvæ. *Thyca Harpalyce* and *Aganippe* are butterflies resembling our "whites," but with rich red spots on the under side of the posterior wings. In the former, the angularity of the tip of the upper wing varies in different individuals of the same brood.

We trust that other of our colonial governments will follow the example set at Melbourne.

Tenth Annual Report of the United States Geological and Geographical Survey of the Territories. Embracing Colorado and parts of Adjacent Territories; being a Report of Progress of the Exploration for the Year 1876. By F. V. HAYDEN. Washington: Government Printing-Office.

THIS bulky and profusely illustrated volume embraces the geology of the district, its mineralogy, topography, archæology, ethnology, and, lastly, its palæontology. In all these departments good work has been done, and important results are not wanting. In a mine on Mount McClellan, near George Town, solidly frozen masses were found at the depth of more than 200 feet from the surface. The walls and roof of the workings are lined with ice-crystals. The cause of this remarkable phenomenon is not known, though it is surmised that chemical changes are in progress in the surrounding rocks which effect a fall of temperature. What these processes may be remains to be discovered. The deposits of tellurets and of uraniferous ores in Colorado are remarkably rich, and splendid specimens have been obtained.

Mr. A. S. Packard, jun., contributes a report on several injurious insects, especially those attacking the cranberry. If the cranberry of America is identical with that of North-eastern Europe we should regard its extinction with much equanimity, and we can assure all who admire this fruit that they can easily reproduce its flavour by adding a stale decoction of nut-galls to the common red currant. An instance is given of a beetle (*Monohammus titillator*) which had remained alive in wood for at least fifteen years.

CORRESPONDENCE.

IMPERFECTIONS OF NATURE.

To the Editor of the Monthly Journal of Science.

Sir,—The writer of the article "Is Nature Perfect?"—which appeared in your number for April—might have usefully controverted a mistaken notion as to the difference between natural objects and works of art. We are often told that the latter, however beautiful they may seem to the naked eye, on examination with a high magnifying power betray a multitude of flaws and defects, whilst the former will bear the most rigid microscopic scrutiny. This is by no means universally the case, not a few natural products displaying a want of symmetry and finish when viewed with a strong lens. To our unaided sight the antennæ of a male gnat appear like beautiful plumes, but if magnified they remind us irresistibly of a bottle-brush. Or take the finger-tip of the most delicate lady; to the eye it seems beautifully smooth and even, whilst with a lens we perceive its surface to be rough, furrowed, and rugged. These cases are by no means singular.—I am, &c.,

Plain Facts.

SOUND AS A NUISANCE.

To the Editor of the Monthly Journal of Science.

Sir,—I fully agree with the writer of an article in your April number, that man's inability to shut out sound at will is a misfortune. But is it not possible that the vibration set up by powerful sounds might have a disturbing action upon our nerves even were we unable to hear? Thus the sensations akin to sea-sickness which some persons, myself included, experience if saluted by the deep notes of a large organ, seem to depend more on the undulations of the floor than upon the sound as such.—I am, &c.,

H. H.

PROCEEDINGS OF SOCIETIES.

Royal Society, *May* 8.—"On the Sensitive State of Electrical Discharges through Rarefied Gases," by William Spottiswoode, P.R.S., and S. Fletcher Moulton. It has frequently been remarked that the luminous column produced by electric discharges in vacuum tubes sometimes displays great sensitiveness on the approach of the finger, or other conductor, to the tube. This is notably the case when with an induction-coil a very rapid break is used, or when with any constant source of electricity an air-spark is interposed in the circuit leading to the tube. The striking character of the phenomena, and the opportunity which they showed for affecting the discharge from the outside during its passage, led the authors of this paper to consider that a special examination of this sensitive state would be desirable. If a conductor be made to approach a tube conveying a sensitive discharge, due to an air-spark in the positive branch of the circuit, a series of effects is produced, of which the feeblest and the strongest are the most pronounced. In the first case, the luminous column is repelled by the conductor; in the second, it is broken into two parts which stretch out in two tongues towards the point on the tube nearest the conductor, while a negative halo appears between them. These effects are proved to be due to the inductive action of the conductor, or more particularly to re-distributions of electricity in it, co-periodic with the air-spark, and not to any permanent charge. Instead, however, of connecting a point on the tube with a large conductor or with earth, it may be connected with one or other terminal of the tube. And a further study of the subject shows that all the phenomena due to action from without may be produced by means of one or other of these connexions. The effects described need not be confined to a single patch or ring of conducting material placed upon the tube; but they may be produced many times over in the same tube by a series of rings arranged at suitable distances. By this means the column may be broken into a series of sections, all terminating with well-defined configurations towards the negative end, and having greater or less length, according to the position of the rings. The authors give evidence, derived mainly from the revolving mirror, and from the discharges of a partially charged Leyden jar, for the following conclusion:—That the passage of the discharge occupies a time sufficiently short in comparison with the interval between the discharges to prevent any interference between successive pulses. Certain experiments are then described which indicate that the discharge is effected, under ordinary circumstances, by the passage through the tube from the air-spark terminal of free electricity, of the same name as the electricity at that terminal. The authors are further led to the conclusion that the discharges at the two terminals of a tube are in the main independent, and that they are each determined primarily by the conditions at their own terminal, and only in a secondary degree by those at the opposite terminal. Having traced the relation between the two parts of the discharge, and having found means for controlling their range and influence, the authors were led to enquire whether there be any experimental evidence of the state of the tube during the occurrence of the discharge. The phenomena appear to require for their interpretation that, in front of the pulse coming from the (positive) air-spark terminal, there is, during the interval between the pulses, a rising negative potential: this is entirely swept out by the pulse as it advances along the tube; after which the process is repeated. The condition of things behind the pulse is more difficult to determine; but an experiment with the telephone gives reason to think that parts of the tube nearer to the non-air-spark end are in a condition to demand relief, before those nearer to the air-spark terminal have ceased to require it. And on this account the discharge may, perhaps, be more nearly represented by a lazy tongs than by a bullet. The marked similarities in the phenomena, and the predisposing circumstances of striation or non-striation, as well as in the terminal peculiarities of the two kinds of discharge, point strongly to the conclusions that all vacuum discharges are disruptive; and that sensitive differ from non-sensitive discharges mainly in the scale of the discontinuity due to the disruptiveness, causing a difference between the two classes of phenomena analogous to that between impulsive and continuous forces in dynamics.

"On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, and the Period of Solar Spot Frequency," by William Ellis, F.R.A.S. In this paper the author draws attention to the long series of magnetical observations which have been made at the Royal Observatory, under the direction of Sir George B. Airy, K.C.B., Astronomer Royal. Commencing in the year 1841, the observations for a few years consisted of eye-readings of the various instruments, made every two hours; since the year 1848 the motions of the magnets have been registered by photography, according to a plan arranged by Mr. Charles Brooke. Attention is briefly drawn to the question of magnetic variations, and to the circumstance that examination of the Greenwich records shows that, in addition to the ordinary diurnal and annual changes, there appears to exist, in the magnetic diurnal ranges, an inequality of marked character and of longer period, resembling in its features the well-established eleven-year sun-spot period. The following are the general conclusions supposed to be derived from the whole inquiry:—

1. That the diurnal ranges of the magnetic elements of declination and horizontal force are subject to a periodical variation, the duration of which is equal to that of the known eleven-year sun-spot period.
2. That the epochs of minimum and maximum of magnetic and sun-spot effect are nearly coincident; the magnetic epochs, on the whole, occurring somewhat later than the corresponding sun-spot epochs. The variations of duration of different periods appear to be similar for both phenomena.
3. That the occasional more sudden outbursts of magnetic and sun-spot energy, extending sometimes over periods of several months, appear to occur nearly simultaneously and progress collaterally.
4. That it seems probable that the annual inequalities of magnetic diurnal range are subject also to periodical variation, being increased at the time of a sun-spot maximum, when the mean diurnal range is increased, and diminished at the time of a sun-spot minimum, when the mean diurnal range is diminished.

Conclusions Nos. 1, 2, and 3 appear to be sufficiently certain, but the evidence in favour of No. 4 is not so decisive.

"On the Results of the Magnetical Observations made by the Officers of the Arctic Expedition, 1875-76," by Staff-Commander E. W. Creak, R.N., attached to the Admiralty Compass Department. This narrative and results form the sequel to the "Memorandum on Terrestrial Magnetism," prepared by Prof. J. C. Adams, M.A., F.R.S., and Capt. F. J. Evans, R.N., F.R.S., published in the "Manual and Instructions for the Arctic Expedition," 1875," suggested by the Arctic Committee of the Royal Society. The *Alert* and *Discovery* left Portsmouth on May 29th, 1875. On September 1st, 1875, the *Alert* reached her winter quarters at Floeberg Beach, lat. 82° 27′ N., long. 61° 22′ W. The magnetic observatory, constructed entirely of snow, was situated at a distance of 200 yards from the ship. It was divided into three distinct houses 27 feet apart, and in these the horizontal force magnetometer, Barrow's dip-circle, and the differential declination magnetometer were secured to snow pedestals. As no stove was used the range of the thermometer registered was from $-10°$ to $+4°$. The *Discovery* took up winter quarters at Discovery Bay, 53 miles S.W. of the *Alert*, in lat. 81° 44′ N., long. 65° 3′ W., on August 26th, 1875. The magnetic observatory house was constructed of wood, and stood on the shore 197 yards from the ship, and the pedestals formed of the ship's binnacles sunk into the ground were found to be very firm. For the differential declination magnetometer an ice house was built on the ice-floe 90 yards from the ship, the magnetometer being frozen on to a pedestal of ice. One of the chief subjects of interest in the magnetical results of the observations at the winter-quarters' observatories is that of the diurnal variation or inequality of the declination, and frequent magnetic disturbances, the latter especially, as the ships wintered in a region remarkable—as is proved—for an absence of brilliant auroras, and in which no connexion was observed between the appearances of that phenomenon and movements of the declinometer magnet. Hourly observations were made with the differential declination magnetometers, and on all occasions of great disturbance special observations at frequent intervals. The greatest range of the declination was observed on February 19th, 1876, about the same hours, at Floeberg Beach and Discovery Bay, reaching 5° 9·4′ and 5° 47·9′ respectively. The smallest range was observed on January 12th, when only 0° 4′ was recorded at the first-named station, and 0° 6·9′ at the last. On an average about every eighth day the higher values of the daily range were attained, and comparing the highest with the lowest scale-reading during the whole period, it shows that the magnet moved over 8° of arc. For computing the mean hourly values of the disturbances of the declination, the formula used by Sir J. H. Lefroy, in the published volume of his "Magnetical and Meteorological Observations at Lake Athabasca," &c., was adopted. From the values of the mean hourly disturbance, without regard to sign, it is found that the disturbing force never ceases, and that in the mean monthly values it *decreases* as the winter solstice is approached, and *increases* rapidly towards the equinox. Taking the difference between the mean easterly and mean westerly disturbances, it is found that the easterly disturbance is both moderate in amount and monthly change when compared with the westerly. The comparison of the disturbances at Kew and the two winter-quarters' observatories of the Arctic Expedition, 1875-76, appear to confirm M. Gauss's conclusion, that "the synchronous disturbances of the same element not only differ widely in amount, but occasionally appear to be even *reversed* in direction." The appearances of auroras and the synchronous movements of the declinometer magnet were subjects of special observation during the stay of the *Alert* and *Discovery* at their winter quarters. On all occasions they were observed to be faint, with none of those brilliant manifestations which are described by our own officers as seen at Point Barrow, and by the Austro-Hungarian Expedition in Franz Josef Land, where the magnetical instruments were so sensibly disturbed. These phenomena were not observed either in the *Alert* or *Discovery*,—especially no connexion between magnetical disturbances and the appearances of auroras could be traced. The following description of the aurora observed on November 21st, 1875, is given by Commander Markham and Lieut. Giffard in their abstract of observations at Floeberg Beach:—"Between 10 and 11 p.m. bright broad streamers of the aurora appeared 10° or 15° above the north horizon, stretching through the zenith, and terminating in an irregular curve about 25° above the south horizon, bearing S.S.W.

During the aurora's greatest brilliancy the magnet was observed during five minutes to be undisturbed." The aurora was visible on forty-nine days between October 22nd, 1875, and February 27th, 1876. The observations from which the foregoing magnetical results have been obtained were made by Commander (now Captain) A. H. Markham and Lieut. G. A. Giffard of the *Alert*, and Lieuts. R. H. Archer and R. B. Fulford, of the *Discovery*.

PHYSICAL SOCIETY, *March* 22, 1879.—Prof. W. G. Adams, President, in the chair.

Capt. Abney, R.E., F.R.S., read a paper "On obtaining Photographic Records of Absorption Spectra." Absorption spectra have hitherto been recorded by the difficult process of hand copying; but the discovery by Capt. Abney of a silver salt sensitive to all rays in different degrees renders the photographic method available. The records thus obtained are photographs of the spectrum of the naked light of the source, and of that of the same light reduced by insertion of the absorbing material in its track, and these are taken parallel, so that the dark absorption lines can be readily compared. Examples of these were thrown on the screen. This method can be used as a new weapon in attacking solar physics, and determining whether or not compound bodies exist in the sun. Absorption spectra to compare with the sun's can be got for compound bodies by burning the matter in question in a flame in front of the slit, and passing a bright light through the flame.

Prof. Guthrie, F.R.S., then read a paper "On the Fracture of Colloids," as illustrated by experiments on the breakage of glass plates, either by pressure or heating at the centre or round the circumference. Circular plates of glass pressed at centre or circumference break in radial lines. However supported, a plate breaks in the same fashion if heated in the same way. If heated in the middle the crack is peak-shaped, like an obelisk on a double pedestal, two cracks forming the outline, with sometimes a third down the middle. The two cracks unite before they reach the edge on one side, and (as afterwards pointed out by Prof. W. G. Adams) the three extremities of the two cracks all meet the edge at right angles to it. The crackage varies with the size and shape of the plates, the flame, and kind of glass; but the type is the same for all. Cracks cross each other. Prof. Guthrie defined a crack as the line where the ratio of cohesion to strain is least, and likened it to the lightning flash.

April 26.—Mr. C. V. Boys gave an account of some experiments made by Dr. Guthrie and himself on the subject of Arago's rotation. The experiments were begun with a view to determine if the drag on a copper disk when a magnet is made to revolve beneath it, or on the magnet if the disk is made to revolve above it, could be made use of for determining the velocity of running machinery. They made the magnet revolve, and obtained the angle of deflection of a disk suspended by a torsion thread (the hair-spring of a watch). They found, as Snow Harris and others found before, that, other things being equal, the drag is directly proportional to the speed, so that if the torsion of the thread could be relied on, and the strength of the magnet did not change, a perfect velocimeter could be constructed. They consider that this method is better than observing the deflection of a magnet over a revolving disk, as in this case they are limited to less than a right angle, and changes in the absolute magnetism of the earth would affect the results. They also determined the effect of change of distance, thickness, diameter, and nature of the disk, &c., their results agreeing with those of former experimenters. They observed that the effect of concentric circular cuts was far greater than that of even many radial cuts; and that when radial sectors were entirely separated from each other the effect was much less than when these were united at the centre. They then experimented on liquids by suspending a sphere or cylinder of the liquid between the poles of a revolving electro-magnet, and succeeded in getting a decided and measurable effect. The importance of this is very great, for they have thus a means of determining the conductivity of liquid electrolytes by currents induced in the liquid without the use of electrodes and without polarisation.

Dr. Guthrie stated that as the push on the liquid is directly proportional to the current quantity, they hope to measure the conductivities of liquids, and connect these to the conductivity of solids through the intervention of mercury.

Prof. Sylvanus Thompson then communicated five laboratory notes from University College, Bristol. The first related to the source of sound in the Bell telephone receiver. Two theories are now being discussed as to this effect: the molar theory regards the motion of the diaphragm in mass as the source of sound; the molecular theory finds it in the molecular motions of the magnetic core of the instrument. Prof. Thompson applied his method of getting magnetic curves with iron filings dusted on gummed glass to this problem. He found that when no currents passed in the telephone the magnetic lines springing from the pole of the magnet are gathered together on the diaphragm opposite, over a central region, which is magnetised lamellarly, or like a magnetic shell. The rim of the plate beyond this region is, however, magnetised radially, and between these two zones there is a neutral circle. It was remarkable, too, that the lines of force touching the plate were bent back around the circle, forming a kind of valley. When the current passed in the coil in a direction so as to reinforce the magnetism, the lines are gathered more closely on the central region of the plate. If the current diminishes the magnetism the lines are, on the other hand, repelled from the plate. The neutral line is also altered. In the first case it shrinks in size, in the second it expands. A small thick disk is wholly magnetised lamellarly; a disk entirely magnetised radially becomes slightly conical in shape. In the actual telephone the disk is flat at the middle and conical at the edges. As the neutral ring shifts, the diaphragm will assume new nodal lines. Dr. Thompson concludes that the molecular theory is not, therefore, necessary to account for the speech of the telephone, although it may assist. As confirming this view, he found that with iron rings round a cardboard diaphragm and an iron centre-piece the enunciation was good, though the *timbre* was altered; whereas with radial pieces of iron on the cardboard the *timbre* was good, but the enunciation bad.

Dr. Thompson next wrote on a saw blade with a magnet, and dusted iron filings on it, which arranged themselves so as to trace the writing. This is usually shown on a steel plate, but a saw retains the virtue for six or eight months. A modification of this experiment due to himself consisted in writing on the blade with one pole of a powerful battery, the other pole being connected to the end of the blade.

The third "note" recommended the use of fine steel fibres, got by breaking iron gauze of thirty-two meshes to the inch, instead of iron filings for exhibiting magnetic lines. The fourth note showed that the lines of force got by filings fixed on cards are magnetic, that of a magnet acting as a magnet. The fifth note explained that solid magnetic "figures" could be got by coating iron filings in shellac to make them light, and floating them in water, or by mixing filings in a soft paste of plaster-of-paris, which could be cut into sections on hardening.

May 10.—Mr. Wollaston explained the construction of Gower's improved form of Bell's speaking telephone. The older form, made of wood or ebonite, is open to the objection that it has a very weak voice, soon gets out of adjustment from changes of temperature, and requires a twisted hand wire, which is liable to break. Gower's form has a comparatively loud utterance, is constant, and does not require to be held in the hand, but may be laid on a table or hung on a wall, a speaking-tube leading from it to the operator's ear or mouth. The "call for attracting attention is also within the Gower telephone itself; whereas in the hand telephone it is an auxiliary apparatus. Every organ of the old telephone has been modified to form the Gower. The magnet in the Gower is of a horse-shoe form, very powerful, the two poles being brought very close together, and each pole is mounted with a small coil of fine wire. The diaphragm is much thicker and larger than the Bell diaphragm. The case is of brass, to expand equably, and a speaking-tube is fitted to the front of the diaphragm. The call consists of a musical reed attached to the diaphragm, so as to be opposite a small slit in the latter. To sound the call it is only necessary to send a sharp puff of wind up the speaking-tube, and the reed gives out a note which is heard throughout a room at the distant end. Speaking and cornet music was transmitted to the instrument exhibited, between the third storey over the hall and the meeting. It was very distinct and audible several feet from the receiver. Speaking done some 30 feet from the transmitter was also sent. Conversation was likewise carried on while considerable noise was being made in the room.

Prof. W. F. Barrett then gave an account of some attempts which he had made to overcome the induction clamour on telephones caused by the ordinary telegraph currents on neighbouring wires. He had tried recently the Bell telephone on a line from Dublin to Armagh, 95 miles long, but the induction noises completely stifled the speaking, whereas the Edison transmitter gave good results. The clamour could be got rid of either by neutralising the induction currents or by eliminating the noises from the speech.

Mr. Wollaston pointed out that a perfect cure for induction on underground wires consisted in twisting the going and returning wire of the telephone circuit round each other.

Mr. Wilson then read a paper "On the Divisibility of the Electric Light by Incandescence." The question of divisibility resolves itself into our being able to divide a single incandescent source into a number of smaller ones giving the same total illumination. The author concludes that this can be done by arranging the subdivided sources in "multiple arc," or parallel circuits, provided the total mass, length, and sectional area of the united sources be the same as in the original single source. The objection that increased radiation from the various sources would diminish the first total of light and heat can be met by making the smaller wires still smaller than is theoretically required so as to generate more heat. The author regards the "voltaic arc" as probably falling under the same law, the mass, however, being smaller in this case.

Dr. Coffin then exhibited a Trouvê Polyscope, which consists of a small hand incandescent platinum wire electric light, designed for illuminating the more inaccessible cavities of the body in surgical examinations. The current is supplied by a Planté secondary battery, and the light is half enclosed in a small silver reflector, fitted with a convenient handle. The apparatus is portable. Dr. Coffin found that it was open to several objections, which he has remedied. Dr. Coffin has superseded the secondary battery by a Leclanché battery of eight elements, made by Messrs. Coxeter and Sons, in which the carbon pole is replaced by a copper plate faced with platinum, and no porous diaphragm is employed. This gives a constant light for hours.

NOTES.

BIOLOGY.

Mr. E. Gittins, of Tivoli, Queensland, in a letter to the Editor, communicates some interesting facts concerning ants. He writes:—"If meat shows the least possible tendency to decompose—and it will do so in the course of twelve hours in summer —the ants will find it, though suspended by a wire or string from the house-top or the top of a tent. The ant perceives decomposing animal matter at a long distance, and does not go exploring for such matter, but goes straight to it from the ant-hill. A snake killed in the Bush is generally placed on the branch of a tree, so as to be seen by travellers, and as soon as decomposition sets in the ants find it, and the flesh is soon carried off to the ant-hill; even their own comrades, when killed, are carried off to the underground cells. They never stay to feed, but they take up the booty and off they go." The writer then describes a number of experiments, showing that portions of meat placed near ant-roads were overlooked till putrefaction set in, and were then eagerly carried off. He remarks that "ants that feed on saccharine matter are as difficult to keep off as the carrion-feeders; they smell the sugar, and endeavour to get at it wherever it may be placed. The largest kind of sugar-ants will feed until the cold air of night comes on, and then fall into a stupor and there remain during the day." We should feel much obliged if our correspondent would determine the two following points:—Whether his meat ants prefer tainted meat to fresh when both are placed equally near, as, *e.g.*, close to one of their roads; and whether they will attack animal matter in an advanced stage of decomposition? It certainly seems that they occupy a more prominent place among "Nature's Scavengers" than has been hitherto supposed.

According to M. Max Cornu, a new malady is attacking plants belonging to the order Rubiaceæ preserved in hot-houses in France. An *Anguillula* deposits its eggs in the roots, which swell and decay, while the plant speedily perishes. The depredator is closely allied to the species which has occasioned so much havoc in the coffee-plantations of Brazil.

M. G. Bonnier has laid before the Academy of Sciences an anatomical and physiological study of the nectaries of plants. He criticises the views of Messrs. Darwin, Müller, Lubbock, &c., and in opposition to their views contends that the dimensions of the corolla, the development of colour in the flowers, of perfumes, spots, and stripes, are not correlated to the formation of nectar, and are independent of the visits of insects. He maintains that, in nectariferous diœcious plants, insects do not visit the male flowers first and the female flowers afterwards, the greater visibility of the former being indifferent. One and the same flower may be visited in various manners by the same insect. The form of a flower may be altered without sensibly modifying the visit of insects. Insects may very often collect the nectar of flowers without effecting their fecundation; the insect-guests of one and the same flower differ according to the quantity of nectar which it produces, and, as this quantity varies with altitude and latitude, the insect-guests of one and the same species differ in different countries. We cannot conclude, from facts observed, that the colour of flowers, the shape of the corollas, &c., are arranged for the exclusion of insects not adapted for cross-fertilisation. In short, there is no reason for admitting a reciprocal adaptation between insects and flowers. There are, further, nectaries without external nectar and all intermediate stages. There are also numerous nectariferous tissues unconnected with flowers. The nectariferous tissues, floral or extra-floral, whether they emit a liquid or not, are special nutritive reserves, in direct relation with the life of the plant. [Without seeking to anticipate the discussion to which this paper must give rise, we cannot avoid pointing out that the facts detailed by M. Bonnier are by no means incompatible with the theory of a mutual adaptation between flowers and insects.]

A writer in "Science Gossip" calls attention to the peculiar position of the mouth in sharks as very difficult of explanation on the Natural Selection hypothesis.

The small annual sum allowed for the purchase of specimens for the zoological department of the British Museum is to be reduced from £1200 to £900.

An anti-vivisection hubbub has sprung up in Germany,, and has now reached a dangerous stage. Unless our Teutonic colleagues are watchful they will some day find research fettered as it is amongst us.

A collection of Lepidoptera from the mouth of the Amur River, exhibited at the February meeting of the Entomological Society, was strikingly European in general character, some of the species being even identical.

M. Lichtenstein has communicated to the Academy of Sciences an account of *Ritsemia pupifera*, a kind of cochineal insect living on the elm, and holding an intermediate position between the Cocci and the Phylloxeræ. In August there appear, on the bark of the elm, small red unisexual lice, with six-jointed antennæ; they evolve a cottony matter, and lay in it not eggs, but pupæ. From these are developed, in the following May, the bisexual individuals, the males of which are apterous, having nine-jointed antennæ and no rostrum. The females, which appear a few days later, much resemble the autumnal generation, but their antennæ are eight-jointed. This mode of reproduction, anthogenesis, has been hitherto traced among the Phylloxeræ and all the Pemphigians. We now recognise it among the Coccidæ.

M. Ch. Richet has studied the mode of contraction of the muscles of the crayfish. He remarks that in the muscles of the tail the shock of contraction is very short, whilst in the muscle of the claws it is much longer than in the muscles of vertebrate animals, excepting the cardiac muscle. The claws of the crayfish, if separated from the body and protected from heat and evaporation, preserve their contractility for more than four days.

Dr. L. Frédéricq, in an inaugural dissertation, shows that the plasma of the blood is made up of fibrinogen, coagulating at 56°; paraglobulin, coagulating at 75°; and serin, at 65°. He finds that the coagulation of blood is a phenomenon in which gases do not intervene in any manner. The carbonic acid is shared between the globules and the plasma (or serum) in the same manner both in circulating blood and in coagulated blood. The red globules are capable of absorbing a notable quantity of carbonic acid.

The "Colonies and India" gives a description of a species of *Coccus* found in Yucatan, and known locally as the niin or neen. It feeds on the mongo, and secretes a large quantity of semi-fluid fat used for various purposes by the natives. If strongly heated a portion evaporates, leaving a plastic waxy matter capable of serving as a varnish. If burnt, it changes to a thick semi-fluid like dissolved caoutchouc, but which solidifies on standing. Numerous applications of these substances are possible.

The editor of the "Scottish Naturalist" recommends, for special observation this season, the possible effects of the abnormal winter and spring upon birds, insects, molluscs, &c.; what species have suffered most; how the reappearance of migratory and hybernating species may be influenced; and especially the degree of colour-variation, and whether such variation, if it occur, tends to melanochroism or leucochroism. Such observations, he justly considers, will aid in throwing light upon the causes influencing the geographical distribution of species.

Mr. R. Ward records, in the "Standard," the discovery of certain fossil remains in excavations now in progress at Charing Cross. The identified specimens are tusks and molars of an extinct elephant (*E. primigenius ?*), a horn of the Irish elk, and bones and teeth of a gigantic ox (*Bos primigenius*).

According to the "American Naturalist" there is reason to fear that the biological department of the great Survey of the Territories is to be abolished. The admirable work which for many years has been done in this department pleads trumpet-tongued against its discontinuance.

Benzol, taken internally, reduces the arterial tension by one-half, and thus aproximates in its action to nicotin. The venous tension is at the same time increased, and the action of the heart accelerated. The secretions are modified; glycosuria is produced in guinea-pigs, but rarely in rabbits, and never in dogs. Phenic acid has been in some cases detected in the blood and the urine.

M. J. Rambosson has communicated to the Academy of Sciences (" Comptes Rendus," April 14th) a paper on the propagation of nervous phenomena. He proposes the following law:—" A purely physical movement may be transformed into a physiological movement, and into a psychic or cerebral movement, by transmission to these different media; and reciprocally a psychic movement may be converted into a physiological and into a physical movement by a corresponding transmission, without any alteration in its nature,—that is to say, it reproduces the same phenomena after all these transmissions and transformations on returning into the same medium.

Dr. Broca has received the brain of an individual of the species *Gorilla Savagii*, aged 2½ years. In the fresh state it weighed 416 grms., which is heavier than the specimen sent in 1876 by Dr. Négre, which weighed only 363 grms., and belonged to another species.

At a recent session of the Anthropological Society of Paris a debate took place on the origin of the blonde race of mankind. Some of the speakers considered that the region of Turkestan was their original seat, whilst others—in particular Madame C. Royer—maintained that they had originated in Europe.

MM. Bancel and Husson have communicated to the Academy of Sciences observations on the phosphorescence of the flesh of the lobster. They consider it due to a fermentation in which carbo- and phospho-hydrogens are liberated, and which is destroyed by putrefaction, just as the bacteria of carbuncle are destroyed by the vibriones of putrefaction.

M. L. Collot has discovered the true *Phylloxera vastatrix* upon *Vitis caribæa*, a wild species of vine found in the forests of Panama, far removed from any vineyards or localities where the true vine (*V. vinifera*) is cultivated. This strongly confirms the opinion that the *Phylloxera* is indigenous in America.

Experiments conducted in the laboratory of Prof. Kühn, of Heidelberg, lead to the conclusion that the so-called "visual purple" of the eye is not essential to vision. It is wanting in several animals which we must consider to be possessed of the sense of sight, and in men it is absent in the " yellow spot,"—that part of the retina in which vision is most distinct.

Dr. A. Horwarth, writing in " Pflüger's Archive " (xvii., Heft I. and II.), contends that—in addition to heat, light, oxygen, and nutrition—rest is needful for the production and maintenance of life. He finds that bacteria are incapable of living in the arteries of animals. Continued motion for twenty four hours hinders the multiplication of *Bacterium termo* and of the *Bacillus* of Cohn, whilst if left in absolute repose they increase rapidly. [This conclusion does not hold good universally; certain low vegetable forms, known familiarly as sewage fungus, seem to flourish best in waters which move swiftly, and are scarcely to be detected in the same waters when perfectly at rest.]

PHYSICS.

From a Report on the Electric Light on the Thames Embankment, which has been submitted to the Metropolitan Board of Works by Sir Joseph Bazalgette and Mr. Keates, we learn that the total cost of twenty lamps per night, of five and a half hours, was about 5·73 pence per lamp per hour. With regard to the value of the light, it is found that with opal shades not less than 59 per cent of the whole light produced was wasted, while with shades made of frosted glass the loss only amounted to 29·9 per cent. Comparing the light produced from gas with the electric light, it is shown that the cost of gas equivalent in power to the electric light is not much over one-half the cost of the latter.

At a recent meeting of the Academy of Sciences M. Jamin submitted a new electric burner, which he also recommended to chemists and physicists as a blowpipe. Two carbons are supported vertically abreast, hinged below, and drawn together at the top by a spring. A current is sent up one (A), down the other (B), then round a rectangular circuit inclosing the two, and passing first round A by current attraction the carbons are drawn apart, and the arc appears at the top and descends gradually, consuming one or both carbons. When the action of the rectangle is sufficient, the arc driven beyond the points is like a gas-flame, and M. Jamin receives it on a piece of lime, magnesium, or zirconium, getting intense light. It is also so hot as to fuse the lime. For the electric light this burner has considerable advantages—such as simplicity, since it requires no mechanism and requires no preliminary preparation beyond a support and the coke points; mechanical economy, since the number of flames is almost doubled; augmentation of light, since each of the new foci is almost twice as powerful as those of the old construction; quality of the light, which is whiter; more advantageous arrangement of the foci, which direct their greatest quantity of light downwards, where it is wanted, instead of up into the air, where it is useless; and, lastly, economy of combustible material.

M. Boudet de Paris has communicated to the Academy a paper "On the Electrical Inscription of Words." The transmitting apparatus is a very sensitive microphonic speaker, the carbons of which, instead of being pressed by a spring, are simply maintained in contact by the pressure of a small piece of paper folded in form of a V. The vibrations of the diaphragm of the receiving apparatus cannot be written, since the movements of the style, however delicate the apparatus, can scarcely be distinguished upon the lamp-black. To enlarge the magnetic vibrations of the receiver, the cover and the diaphragm of a Bell's telephone are taken away, and on the wood of the instrument there is fixed the end of a small, stiff, steel spring. The other end of the spring abuts on the surface of the magnetic nucleus, surrounded by its coil; to this extremity is soldered a small mass of soft iron weighing about 10 grms., and upon this mass, and in the produced line of the axis of the spring, is fixed a light style of bamboo, 10 centimetres in length, and terminating in a slender whalebone pen.

An improved siren with an electro-magnetic regulator is described by M. Bourbouze in the "Comptes Rendus." By means of this instrument sound can be made to pass from 8162 vibrations per second, through all the intermediary notes, to 128 vibrations.

The following resolution respecting the standard unit of Micrometry has been passed by the Royal Microscopical Society:—"That, in the opinion of this Society, the 1-100th of a millimetre is too large a unit for micrometric measurements, and that it is not expedient at present to prescribe by any formal resolution the adoption of a fixed standard for Micrometry."

The oil immersion objective has been made successfully by Messrs. Powell and Lealand.

Herr Zeiss proposes the following fluids as substitutes for oil of cedar wood for use with oil immersion objectives:—

1. Chloride of cadmium in glycerin ($CdCl_2$), 1·504.
2. Copaiva balsam oil, 1·504.
3. Chloride of zinc in water ($ZnCl_2$), 1·504.
4. Sulpho-carbolate of zinc in glycerin, 1·501.

Prof. Abbe considers the chloride of cadmium in glycerin very good optically, but somewhat too thick for convenient use. The oil of copaiva balsam is in every respect equal to oil of cedar wood, but not quite so fluid, and therefore better adapted to objectives of such large working distance as the ⅛th. Chloride of zinc is not suited to prolonged observations, as after a few minutes' use it deposits small crystals on the slide and front lens: great care is required in the use of this and other aqueous fluids, lest the brass setting of the objective should be corroded. Platinum has been suggested as a material for mounting the front lens, but Herr Zeiss has found that its want of rigidity is fatal to its employment, as it will not stand when turned to the mere shell required.

PHILOSOPHICAL SOCIETY OF GLASGOW

THE MONTHLY

JOURNAL OF SCIENCE.

JULY, 1879.

I. THE ANOMALOUS SEASON.

THE exceptional weather which the British Islands, along with Western Europe as a whole, have experienced during the last eight months, makes meteorologists of us all. Men are eagerly asking whether the present season is a calamity hitherto without precedent, showing perhaps a lasting deterioration of our climate, and announcing the return of a glacial epoch, or a mere periodical phenomenon recurring at fixed intervals? If we accept the latter, or cyclical, hypothesis, of how many years must we suppose the cycle to consist? There is a widespread tendency to connect recurrent phenomena, famines, pestilences, inundations, storms, and even commercial depressions, with the periodicity of the sun's spots—a term between ten and eleven years. That many facts speak in favour of such a supposition is evident, and it may therefore be worth our while to consider whether a pre-eminently severe and ungenial season may be looked for every ten or eleven years. We must ask, in the first place, how, in looking back over the past, are we to recognise these bad seasons? Mere meteorological returns, average, maximum and minimum temperatures, quantity of rainfall, and even hours of sunshine and proportions of the equatorial and the polar currents, throw but an imperfect light on the subject. It is not so much the quantity of cold, of wind, of rain, and of clouded skies, as their manner of distribution, which decides whether a given season shall be pleasant and fruitful or the reverse. The best criterion of the character of the winter and the spring must be found, we think, in certain phenomena of the organic world, such as the leafing and flowering of trees. In like manner the nature of the summer and the autumn may be most conveniently judged by the ripening of seeds and fruits, by their abundance or scarcity, and their quality. This is merely extending a principle fully recognised in climatology. If we say that in a certain district the sugar-cane, or the banana, or the cacao, or the date-tree flourishes, everyone versed in botanical geology will at once form a fairly precise notion of its climate. In like manner if we say that in England, in a certain year, the sloe was in full bloom on May 13th, we at once characterise that season.

What evidence, therefore, has the vegetable life of the country to give for or against a recurrence of bad seasons every ten or eleven years, between each pair of which a strikingly good season is interposed, though not generally in the midway?

Within the past forty years there have been four, or perhaps five, signally inclement years: the winter 1844-45, with the succeeding spring and summer; 1854-55; 1860-61; 1870-71 (a less decided case); and lastly, 1878-79. The first of these seasons showed the ordinary features of a prolonged winter, a backward spring, and a wet and cold summer. In some parts of the north of England skating was practicable in March. The Rev. L. Jenyns—who has tabulated the earliest, average, and latest times of a number of organic phenomena, as observed in the years from 1820 to 1831—notes that in 1845 these occurrences were later than his latest dates, the differences being, however, variable in their amount. Hence we learn that different plants, as might be expected, are very unequally affected by bad weather in their times of leafing or flowering. In a bad season there is not merely a general translation of periodic phenomena to a later part of the season, but there is also respective displacement. Animals, we must observe, are a less trustworthy guide than plants, because they more readily escape our notice. We may, for instance, register the first appearance of some bee or butterfly on a certain day, whereas had we been in another field we might have observed it a week earlier. The summer of 1845 fully maintained the character of the earlier portion of the year.

The season 1854-55 was in almost every respect similar. The cold set in early and continued late. Frosts, snowstorms, and sleet continued into June, and on the morning of July 1st a frost cut off the potato-plants over much of the north and east of England. No fine weather ever came, and the harvest was late and deficient in quantity and quality.

In 1860-61 all these characters were still further intensified. Already in October, 1860, so severe a frost occurred that rocks and railway embankments were fringed with icicles. On the morning of Christmas-day was observed the severest cold probably ever experienced in England. Large well-established apple-trees, and even oaks of a yard in girth, were killed. Through the spring and summer the polar current still retained its ascendancy, and in many parts of the north of England frost was experienced almost every night in the month of July. The corn rotted where it had grown, and was ploughed into the ground in the ensuing season.

The claims of the season 1870-71 to rank in this evil catalogue are not unquestionable. It was undoubtedly the severest winter between 1860-61 and 1878-79, and the snow which fell on December 22nd lay unmelted into February. There was, further, no summer weather till the beginning of August. But November and the earlier part of December, 1870, were mild and genial; and in March, April, and May, notwithstanding an excess of easterly winds, the leaves and flowers were not far behind the average in their time of appearance.

We now come to the present season, the characteristics of which are too fresh in the public mind to need recapitulation. The following facts will show the effects upon vegetation :—

	Mean Time.	1879.
Sloe and plum flower ...	April 7	May 13
Maple in leaf	April 10	May 24
Lilac flowers	April 30	June 3
Ash in leaf	April 30	May 25
Oak	May 2	May 30
Horse-chesnut flowers ...	May 3	June 12
Whitethorn flowers... ...	May 5	June 4 (a few flowers in sheltered places).

The animal world is much less gravely affected. The cuckoo was first heard and seen here on April 25th, two days before its mean time of appearance, and the swallows returned to us on April 20th, only one day behind its average. The nightingale, also, was singing at its accustomed season, though among branches nearly leafless. These facts, we submit, show that migratory birds do not hasten or delay the time of leaving their winter-station by reason of any mysterious knowledge of the weather prevailing in the country whither they are bound. Among insects *Gonopteryx Rhamni*, *Vanessa Urticæ*, and *V. Io* were seen as early as March 7th, their respective average times, according to Jenyns, being March 10th, 27th, and 31st. *Euchloe cardamines* appeared this year on June 5th—nearly a month behind its average time. *Callidium violaceum* was seen this year exactly at the same time as in the two previous seasons. Aphides have already descended in swarms upon fruit-trees, and are committing serious damages. Hence we may argue that long and severe winters have little direct influence upon insect life, and that there is consequently little hope of frost ridding us of noxious species. Not a few countries whose winter temperature is far below our own are much richer both in insect species and individuals than in England.

If we next look to the intervals between the five exceptionally bad seasons we have mentioned, we find them separated respectively by 10, 6, 10, and 8 years. If we reject the claims of 1870-71 we have, of course, instead of the two last mentioned, a single space of 18 years. Before attempting any comment upon these figures it may be useful to turn to the exceptionally fine seasons. As a starting-point we may take 1868 as utterly unquestionable in its character. It was preceded by a winter milder and shorter than the average. The equatorial current early got and maintained the upper hand. March was milder than an ordinary May, and the middle of July brought a splendid wheat-harvest. In October wet weather set in, but unattended with cold. Christmas-day was warm and genial, with primroses and violets in bloom. The winter was not marked by either severe, lasting, or often-repeated cold. The two following summers and the intervening winter, though inferior to the splendid season of 1868, were, to speak in the most guarded manner, a very fair average. Since then no summer of an exceptionally fine character has occurred, so that we have the whole period between 1870-71 and 1878-79 without an intervening season of heat.

Turning to the past, if we wish to find another year similar to 1868 we must go back forty-three years, to 1825, when the wheat-harvest also fell in July. Still there have been in the meantime seasons very much superior to anything experienced since 1869. Thus the summer of 1857 was dry and warm. In the succeeding winter the only frost experienced was in February, and was of no long duration. The summer of 1858 was even finer than its predecessor, and was followed by winter free from frost and snow, save from about November 12th to 17th. Here, therefore, we have two epochs of heat separated by an interval of about ten years. Had the series been carried out we should have had another warm epoch in the year 1878 or 1879, or in both, followed by unusual cold in 1880-81.

It will be apparent, on examining the above facts, that an unusually warm summer does not stand alone. As a rule it is preceded or followed by mild winters, and may be regarded as a culminating point from which the temperature descends on either side through a total space of about two years. With exceptionally cold and prolonged winters the case is inversely similar; they are either ushered in or succeeded by chilly, wet summers, and the depression of temperature is rarely limited to a single season. Thus the summer of 1862, though not absolutely frosty like its predecessor, was still cool, and not until 1863 had we any true summer weather.

Much of this becomes intelligible if we remember that the character of the seasons in Western Europe is more immediately at least determined by the conflict of the two great atmospheric currents, the polar and the equatorial. In what may be called average years the opponents are about equal in strength, and have their battle-ground over the British Islands. Hence the proverbial fickleness of our climate, the cloudiness of our heavens, and constant possibility of warm weather at Christmas, or of frost in summer or early autumn, according as one or other of these aërial rivals wins a temporary advantage. Sometimes, however, though rarely, the equatorial current gets decidedly the upper hand, as in 1868, and retains it for a year or more. In this case the battle-ground is transferred to Norway, whilst we are deeply and continually immersed in southerly winds. The weather then is not merely warm, but dry, for these winds do not deposit their moisture unless chilled by the near proximity of a polar current of air. More commonly, however, as in the present season and in 1861, the polar current gets and maintains a long superiority. The conflict is then transferred to Spain, and we are completely plunged in icy winds. If a southerly or south-westerly air blows for a short time, its stock of moisture is immediately precipitated by contact with the chilled earth or by polar currents in the upper regions of the air, so that the only result is a torrent of rain followed by a fresh rush of the north-east wind. Even the storms which cross over from America fail to give any relief; they are deflected southwards by some unknown cause, and strike the coasts of France, Spain, and Morocco.

But these considerations, after all, take us but a very little way; we ask why should the equatorial current prevail in one season and the polar in another? Why is either, when supreme, apt to retain its sovereignty so long? Why is the change from a cold to a warm epoch, or inversely, generally effected about the autumnal equinox? Although the cold and the warm seasons by no means exactly and invariably succeed each other at intervals of from ten to eleven years,—or otherwise we should now be enjoying a recurrence of 1868,—we feel by no means free to maintain that the alternation in question is not connected with the periodicity of the solar spots. The exceptions may very possibly be residual phenomena due to the intervention of some as yet unsuspected cause, which may either displace the epochs of greatest heat and cold or may obliterate one of them altogether.

A practical consideration is how the approach of an abnormal season may be foreseen. If northerly winds begin in October, and continue with little intermission till the end of the year, there is already room for grave apprehension. But if, after an early commencement of winter, there is no decided change before the end of February, a cold late spring and a chilly summer become almost a matter of certainty. Hence it will be seen that the writer is not one of those who expect heat and drought as characteristics of the approaching summer.

We may at any rate safely say that a severe and prolonged winter is far from announcing a warm summer. Nor is it safe to conclude that in a late spring the fruit will escape the effects of frosty nights. Such frosts in a late season simply occur in June instead of May.

II. THE HISTORY OF VESUVIUS DURING THE LAST TEN YEARS.

By G. F. Rodwell, F.R.A.S., F.C.S.,
Science Master in Marlborough College.

THE earlier eruptions of Vesuvius were of a violently paroxysmal character. The pent-up forces did not discharge themselves little by little at frequent intervals of time, but a long period of tranquillity, or of slight dynamic activity, was followed by one of great eruption. Prior to the eruption of A.D. 79 the mountain was not even regarded as an active volcano. From this period until the year 1139 there was no eruption of any magnitude, and then more than five hundred years elapsed before the mighty outburst of 1631, which entirely changed both the shape of the mountain and the aspect of the surrounding country. From this time the eruptions occur more frequently. During the 18th century the mountain was rarely in a state of rest, and more than four-fifths of the recorded eruptions occurred in this and the present century. These, as far as, and including the eruption of 1868, have been described so fully by Prof. Phillips in his admirable account of the mountain, that we need only refer to them, and may pass on at once to the history of Vesuvius since 1868.

The eruption of 1868 terminated in November, and the mountain remained quiet till the end of December, 1870, with the exception of a few fumeroles of the crater, which emitted gases, and deposited near their mouths sublimations of chloride and sulphide of copper, and sulphide of potassium. Early in 1871 the seismograph of the Observatory showed signs of disturbance; slight detonations occurred within the crater, and incandescent particles were discharged in small quantity. On January 13th an aperture made its appearance on the northern side of the cone, from which a little lava issued, and around which a small cone was soon formed. A red-coloured smoke soon afterwards issued from it, and incandescent scoriæ were ejected, while the detonations proceeding from the great crater were louder and more frequent. Lava continued to flow from the small cone, at short intervals, until the beginning of March, when the cone subsided, leaving a cavity about 30 feet in depth, the walls of which were covered with sublimations, while at the bottom a small cone 6 feet in diameter made its appearance, and ejected smoke and steam with a hissing noise. This lilliputian cone increased in size by the accumulation upon its sides of the scoriæ which it ejected, until it rose to a height of 16 feet above the brim of the cavity; it then commenced to discharge lava, which flowed into the Fossa della Vetrana to a distance of about 1000 feet from the new cone. This lava is very leucitic in appearance, and appears to consist almost entirely, according to Palmieri, of minute crystals of leucite imbedded in a homogeneous paste. The stream continued to flow slowly in the direction of Canteroni for several months.

In October, 1871, another small crater opened near the edge of the large cone of Vesuvius, and in November, lava was emitted from the great crater, which ran down the western slopes of the cone. In the following January the crater of October became active; loud bellowings were heard, and it emitted lava and projectiles. In February the activity somewhat abated, but in March an opening was made in the side of the cone, from which a good deal of lava was discharged into the Atrio del Cavallo. The stream flowed for a week, and then ceased. On April 23rd the seismograph showed a good deal of disturbance, and on the 24th a considerable quantity of lava was emitted, and it continued to flow for two days. Early on the morning of April 26th a number of persons went with inexperienced guides to see the new lava streams: as they were proceeding towards the summit the great cone was suddenly rent open in the north-westerly direction, commencing at the new cone and extending to the Atrio del Cavallo, and a copious torrent of lava proceeded from the fissure. Soon afterwards two small craters appeared near the summit of the principal cone, which discharged incandescent particles and quantities of white ash. The whole series of effects took place so suddenly that eight medical students who had approached too near to the scene of action were overwhelmed by the lava, and a number of persons were injured by the hot ashes and scoriæ. The great fissure of the principal cone was both broad and deep, and it extended for about 1000 feet into the Atrio. A ridge of little hills, like a miniature chain of mountains, marked the course of the fissure. The highest point of the ridge was about 150 feet above the plain. A smaller fissure opened on the south side of the cone, and a small stream of lava flowed from it towards Camaldoli.

The lava which flowed from the great fissure on the north-west side of the mountain divided itself into two branches, the larger of which was 875 yards in width, and it flowed to a distance of 1425 yards in the Fossa della Vetrana in three hours. It then descended into the Fossa di Faraone, where it again divided into two streams, one of which ran over the lava of 1868 in the Piano delle Novelle, while the other flowed through the Fossa di Faraglione over the lava of 1855, and reached the villages of Massa and S. Sebastiano. The stream passed between them, and afterwards continued its course through an artificially constructed channel, until it reached some highly cultivated ground, which it overwhelmed. The lava moved with great rapidity; between 10 and 11 a.m. it traversed more than 3 miles, and covered an area of 2 square miles. The supply of lava fortunately ceased at this source, otherwise in less than twenty-four hours the stream would have reached Naples and flowed into the sea. About one-third of Massa was destroyed, and less than one-fourth of S. Sebastiano. The stream which separates the two villages is more than half a mile in breadth and 20 feet in depth.

On the night of April 26th, 1872, the Vesuvius Observatory lay between two streams of lava, and the inmates found the radiated heat very oppressive. The lava seemed to ooze through the sides of the cone. The two craters which had opened near the edge of the great crater gave vent to loud bellowings, produced great clouds of smoke and ashes, and projected volcanic bombs to a height which Palmieri estimated at nearly 1500 feet. This would imply an initial velocity of 655 feet per second. The white ashes were carried by the wind as far as Cosenza. Dark-coloured sand, lapilli, fragments of scoriæ, and much smoke were at intervals emitted during the eruption. Many people fled in terror from Naples; Palmieri remained, however, at his post in the Observatory.

On the morning of April 27th the lava ceased to flow. It was noticed that in three several places in the lava bed great masses of smoke were emitted, and projectiles were driven into the air, as if small volcanic rents were existing beneath. But this effect, which sometimes lasted from fifteen to twenty minutes, was probably due to the sudden evolution of gases produced by the passage of the lava over wet places. On the evening of the 27th the pine-tree cloud of smoke from the crater became darker in colour, and frequent flashes of lightning were emitted. On the following day the pile of ashes and lapilli was so great that it darkened the air, and great terror pervaded the inhabitants of Resina, Portici, and Naples. On the 29th a strong wind blew from the crater towards the Observatory, and the scoriæ broke some of the windows. Ashes accumulated to a depth of 7½ inches on the roof. Towards midnight the noise of the craters diminished; the next day the detonations were few and far between; and by May 1st, 1872, the eruption was completely at an end.

On ascending to the summit of the cone Palmieri found the great crater divided into two parts by a wall with vertical sides 750 feet in height. The interior showed no sublimations, but alternate beds of scoriæ and compact lava. Fumeroles emitting sulphurous anhydride, hydrochloric acid, and sulphuretted hydrogen at a temperature of 150° C. were found at the outside edge of the crater.

Palmieri regards the great eruption of April 26th, 1872, as the last phase of the eruption which commenced early in 1871. He calculates that the total amount of lava emitted amounts approximately to 20,000,000 cubic metres, about three-fifths of which was deposited on pre-existing lavas. Its destruction was estimated at 3,000,000 of lire. Among the products of the eruption were noted:—Black oxide of copper, chloride of lead, chloride of iron, chloride of ammonium, chloride of calcium, and chloride of sodium. Traces of the chlorides of thallium and lithium were also detected.

For some months after the dying-out of the eruption of 1872 Vesuvius remained in a quiescent state, and the only manifest sign of the hidden fires appeared in the few fumeroles on the floor of the crater. These, however, gradually increased in number, the temperature rose, and steam and carbonic acid were abundantly evolved. Afterwards the carbonic acid diminished, and sulphurous acid made its appearance. Early in January, 1875, the writer ascended the mountain, and found the crater full of steam and sulphurous acid. It was quite impossible to descend into it. As the wind from time to time blew the fumes over the edge of the crater we suffered from a feeling of suffocation, and violent coughing was produced. One of our party wore a pea-jacket of fluffy serge dyed with indigo, and the sulphurous acid was in sufficient quantity to bleach it superficially.

Later in the year hydrochloric acid appeared—a sure sign that the volcanic activity was increasing. This gas is produced by the action of silicates at a red-heat upon salt in the presence of aqueous vapour, and is a constant product of intense volcanic action in all parts of the world. Towards the end of the year (December 18th) a deep fissure opened in the floor of the crater, at the bottom of which molten lava could be seen. The surging liquid mass afterwards rose to the top of the fissure, and a cone was thrown up over the point of greatest activity. From this lava flowed in small quantities and at intervals during 1876, 1877, and 1878. It overspread, and gradually raised, the floor of the crater, until, on November 18th, 1878, it rose to the edge of the lowest portion of the rim of the crater on the north-west side of the mountain, and ran down the great cone towards the Atrio del Cavallo. The flow soon ceased, but the new cone continued to exhibit considerable dynamic activity, and is still ejecting scoriæ and evolving volumes of steam.

When the writer visited the scene of the new eruption, a few weeks after its commencement (December 29th, 1878), the cone exhibited considerable activity. We ascended with a guide from Resina, and the ascent thence to the foot of the cone occupied an hour and three-quarters, while that of the cone itself—which slopes at an angle of 32°, and is composed of loose ashes—occupied fifty-five minutes. The cone on the north side presented precisely the same aspect as in 1875. On arriving at the rim of the crater we turned to the west, until we came to a portion which had been broken down by the recent flow of lava. Near this north-western extremity of the rim we turned aside, and descended to the floor of the crater by a very steep path. At the bottom of the crater we at once found ourselves upon the new lava, which in many places was red-hot a few feet beneath the surface. Dense fumes of hydrochloric acid escaped from fissures in the lava nearest to the new cone, and certain large cavernous places in the lava were coated with brilliant red and yellow incrustations of sesquichloride of iron—often erroneously described as sulphur. Masses of nearly white porous lava were sometimes found near the cone. These had probably been submitted to the action of hydrochloric acid at a high temperature, which had removed the iron from the mass in the form of chloride. Such decomposed masses consist almost entirely of silicates of lime and alumina: they may be artificially produced by passing hydrochloric acid over volcanic scoriæ heated to redness in a porcelain tube.

Palmieri has detected chloride of calcium and chloride of lithium among the products of the last eruption; also boracic acid and various sulphates. In specimens collected in December we have found, among other substances, chloride of aluminium, and microscopic crystals of a compound of copper, probably chloride. The lava is black and lustrous, and it contains a good deal of leucite. It does not resemble that which was emitted in 1872.

The new cone of November, 1878, presents several points of great interest. It is so small, and its dynamic activity is of such a character, that it is possible to approach quite close to it, and thus to observe the exact nature of its action. We approached within a dozen yards of its crater, and stood nearly on a level with it on a heap of scoriæ, which sometimes shook beneath our feet. A peculiar loud surging sound was heard within the crater, such as we have heard within the Strokkr Geysir, in Iceland, but much louder and more intense—in fact the difference that one would expect between violently agitated water and violently agitated molten lava. At intervals loud detonations occurred, the ground shook under our feet, and a thousand red-hot pieces of scoriæ were shot on to a height of several hundred feet. The majority of these fall back into the crater, or on that side of the cone away from the wind; but sometimes, on the occurrence of an unusually violent outburst, the masses would be scattered far and wide. Two such masses fell unpleasantly near; the second, which must have weighed at least a pound and a half, fell within 4 feet of us. They can generally be avoided if they come down singly, but of course if a shower descends the best thing to do is to run for it. A guide had been killed by a falling projectile fifteen days before our visit. The new cone presented altogether one of the most remarkable sights we have ever witnessed; the earth-shakings, concussions, and detonations; the showers of red-hot scoriæ, rivalling any fireworks; the clouds of steam and lurid smoke, and the entire surroundings—all contributed to make it a remarkable sight, and to prove that a really great eruption, like that of 1631 or 1872, must be altogether the most wonderful sight in Nature.

In the last number of the "Bullettino del Vulcanismo Italiano," which was published a few weeks ago, we look in vain for any news of Vesuvius. In the "Cronaca Vesuviana" an account of our visit in December (communicated to "Nature," and very inaccurately translated for "La Nature") is reprinted with all the errors. Of the products Prof. Palmieri writes—"Le sublimazioni, peculiare obbieto di studi scientifici, sottoposte continuamente all' azione delle pioggie, non han potuto accumularsi in gran copia. Tra' soliti cloruri e solfati si è confermato allo spettroscopio la presenza dell' acido borico e del litio." A correspondent of "La Nature" writes from Naples on January 24:—"L'éruption du Vésuve vient de recommencer. Ce soir a 7 heures 30 minutes, la coulée est superbe, la flamme plus vive et le torrent de feu plus animé qu'il ne l'a été pendant toute la période antécédent. . . L'éruption continue au grand cône; au sortir de ce qu'on appelle ici la 'Fenètre' la lave se déverse en deux courants dont l'un suit le lit de la coulée antérieure vers Somma, et dont l'autre se dirige vers Naples." No augmention of intensity seems to have taken place since the above was written, and the eruption which commenced last November still justifies Palmieri's appellation of *una modesta eruzione.*

A record of the history of Vesuvius during the last decade would be very incomplete without reference to a notable addition to the literature of the mountain, in the "Report on the Chemical, Mineralogical, and Microscopical Characters of the Lavas of Vesuvius from 1631 to 1868," by Profs. Haughton and Hull, printed in the "Transactions of the Royal Irish Academy" for March, 1876. Herein the authors have endeavoured to discuss the composition of the Vesuvian lavas as a branch of the Indeterminate Analysis, founded on what Prof. Haughton calls "the principle of least paste." These lavas are mainly composed of nine minerals, viz., leucite, plagioclase, magnetite, olivine, augite, hornblende, mica, nepheline, and sodalite, of known composition, together with an unknown quantity of a paste of unknown composition. The object of the authors is to group the constituents given in the ultimate analysis of the lavas into their several compounds; for example, to say how much of the silica exists in leucite, how much in augite, and so on. Of course it is well nigh impossible to say how the various constituents of lava are grouped in the form of different distinct minerals, but we can gain some idea of the abundance or scarcity of certain minerals in a lava by the microscopic analysis. In applying the principle of least paste Prof. Haughton has proceeded on the assumption that "of the numerous solutions possible that one will occur in Nature which involves the largest amount of Definite Minerals and the least amount of Indefinite Paste."

The following proximate and ultimate analyses will illustrate the result of Prof. Haughton's work:—

	Lava de Granatello. Vesuvius, 1631.	Lava Stalactite. Vesuvius, 1868.
Silica	48·54	46·58
Alumina...	14·86	20·00
Iron peroxide ...	4·17	3·20
Lime	11·89	9·09
Magnesia	5·75	3·16
Soda	2·71	2·74
Potash	6·45	7·35
Iron protoxide ...	4·82	5·69
Titanium protoxide	0·21	0·27
Manganese protoxide	1·18	1·07
Phosphoric acid ...	0·18	Not determined
Fluorine...	None	None
Chlorine...	Not determined	Not determined
Water	0·16	0·32
	100·92	99·47

The same lavas with the constituents grouped according to the principle of least paste:—

	Lava de Granatello.	Lava Stalactite.
Leucite	33·60	41·80
Anorthite	0·60	18·20
Magnetite	4·45	3·75
Olivine	trace	—
Augite	41·20	22·60
Hornblende	trace	—
Nepheline	10·00	—
Sodalite	trace	6·04
Apatite	0·44	—
Paste	9·71	7·61
	100·00	100·00

The percentage composition of the minimum paste in the case of the Lava di Granatello is—

Silica...	45·0
Lime	27·0
Protoxides...	28·0
	100·00

And of the other—

Silica	21·6
Lime	13·2
Iron protoxide	65·2
	100·00

The details of the calculations are given at length in the memoir, which is accompanied by a description of the microscopical examination of thin sections of the lavas by Prof. Hull, and a coloured plate giving the more marked peculiarities. We trust that the principle of least paste will hereafter be applied to the lavas of Etna, the Lipari Islands, Iceland, and Hawaii.

Addendum.—Towards the end of last month, when Etna was in a state of violent eruption, Vesuvius showed signs of an increase in dynamic activity. A telegram from Naples, dated June 11th, states that "A slight eruption has commenced to-day from Mount Vesuvius, and is gradually increasing." A private letter from Naples, dated June 16th, contains the last piece of news which we have been able to collect concerning the present condition of Vesuvius:—"It shows some small activity at the summit. The upper crater is full of liquid lava, which at times boils over, and sometimes also is thrown up, but no great amount of force is to be observed."—R.

III. A NEW THEORY OF DEW.

INVESTIGATIONS which Prof. Levi Stockbridge has made at the Amherst Agricultural College, upon the comparative temperature of soil and air, and the disposition of dew upon the earth and plants, have led him to conclusions very different from those commonly received in regard to the formation of dew. It is usually held that dew is the moisture of the air, condensed through contact with objects of a lower temperature, and that it does not form till radiation has reduced the temperature of the earth and other objects below that of the atmosphere. The experiments referred to seem to indicate that, as regards objects in the immediate vicinity of the earth, at least, the process is the converse of this, viz., that dew is the result of condensation, by the air, of warm vapour as it rises from the soil. The course of experiments from which this novel scientific theory was deduced is outlined below.

The basis of the theory is the discovery that in summer the average temperature of the earth at night is higher than that of the atmosphere. The temperature of the earth in an enclosed space on a level with the surrounding soil, and the temperature of the air, were taken at the warmest time of day and the coldest time of night for several months, and the average temperature of the air for the season was found to be 72·940°, and that of the soil 72·061°. But the average temperature of the air at night was 49·664°, and that of the soil 56·370°, the earth thus averaging at night over 6° warmer than the atmosphere. The temperature of the soil and air at night was also taken at various points within 10 miles of the college, on all kinds of grass land and bare soil, and in the forest, and the same facts were obtained, the soil being at all times warmer at night than the air.

These results led to experiments on dew-fall. Two boxes, each of a cubic foot capacity, were filled with soil without disturbing its particles or disarranging its strata; one receiving absorbent, retentive loam, and the other peat. These boxes were placed in a trench, in an open field, level with the surrounding ground, and exposed to the weather. Through the month of June they were weighed night and morning, and unless there was a rain in the night they uniformly weighed less in the morning than at night, the loss being from 1 to 3 ounces for the loam, and 1 to 4 ounces for the peat. This, Prof. Stockbridge thought, indicated that the soil at night gave forth water, and that the moisture found on the surface of a field in the morning came from a deeper soil rather than from the air. Other similar experiments followed. In one a cabbage-plant was enclosed in an air-tight tin case. Where the stem of the plant protruded through the top of the case, wax was used to make it impossible for moisture to escape through the leaves. The can was first kept within doors and weighed night and morning, when it always showed a loss during the night of 1·21 to 1·78 grms. When left out-doors at night, with the can wrapped in cloths to prevent moisture reaching it, the loss was from 0·55 grm. to 4·23 grms., showing a loss even when there was moisture or dew on the leaves.

These experiments, continued through the season, gave Prof. Stockbridge these proofs of his proposition that the dew on the ground in the summer is the condensation of vapour that rises from the earth:—1. The vapour of the soil is much warmer at night than the air, and would be condensed by it. 2. Vapour from the soil is soon diffused and equalised in the whole atmosphere, but in largest proportion when evaporation is taking place near the surface of the soil; and, other things being equal, plants nearest the earth have the most dew. 3. Dew under haycocks, boards, and like objects on the ground, could receive it from no other source.—*Kansas City Review of Science and Industry.*

IV. ARTIFICIAL FLIGHT.*

By F. W. Brearey,

Honorary Secretary to the Aëronautical Society.

IN the Paper which I read before the Aëronautical Society at the General Meeting in 1878 I stated that, with respect to those of my models which are actuated by wing-movement, I had found a difficulty of sustaining the weight which appertains to the living example. But from experiments which I have lately made I find that the weight to be sustained depends upon power and strength of material employed, and also upon the right application of that power. For instance, in the wing-action of the bird, that portion of the pectoral muscle which depresses the wing is considerably more powerful than that which elevates it, because its function is to sustain the whole weight of the bird by impact upon the air. In my models I attain that action by affixing india-rubber cord, more or less in tension (according to the power used), underneath the wings, so that when the wings are elevated there shall be a strong downward impetus. There is, however, a disadvantage not shared by the bird,—the upward stroke in the model absorbs much power in working against the tension of the india-rubber underneath. Given that power, and with material to endure the pressure considerable weight can no doubt be sustained, What immediately concerns us, however, is this:—Can the weight equal to that of a man and the additional power necessary to propel him be sustained by any material which man can construct light enough and strong enough?

---

* A Paper read before the Aëronautical Society of Great Britain.

Mr. Wenham, in his Paper read at the first meeting of the Aëronautical Society, says—"Having remarked how thin a stratum of air is displaced beneath the wings of a bird in rapid flight, it follows that, in order to obtain the necessary *length* of plane for supporting heavy weights, the surfaces may be superposed, or placed in parallel rows, with an interval between them. A dozen pelicans may fly one above the other without material impediment, as if framed together; and it is thus shown how two hundredweight may be supported in a transverse distance of only 10 feet." But I think that the difficulty of actuating the wings of these twelve mechanically imitated pelicans would present as great a difficulty as the vibration and construction of wings of the dimension of "60 feet from end to end, and 4 feet across at the widest part," which in the same Paper Mr. Wenham estimates as necessary to support the weight of a man. If to this we add the weight of motive power—for it is evident that man does not possess the necessary power—we might, but for other encouraging reasons, give up the hope of his ever being able to navigate the air, because its possibility, in the way that a bird flies, is held to rest either upon the necessary surface extended laterally or else superposed, and in either case—were the difficulty overcome—there would remain the important question of balance, which in the bird is maintained, I believe, similarly to that unconscious muscular action in the sole of the foot and leg, when a man stands in an upright position. Under such a surface, therefore, there would be, I maintain, no safety.

I grant that a light and very powerful motor would favourably alter some of these conditions, because the rapid vibration of a much smaller surface would effect the same result, but increase of strength of structure would be necessary.

I had hoped that some rotatory action, which would have presented to the air in rapid succession all the equivalents of flight by wing-surface, would have solved the difficulty; but the only attempt hitherto made in that direction has been effected by Mr. Moy, at a cost of several hundred pounds. The velocity which was requisite in its preliminary run was fatal to the attempt, because it could not be attained even if the machine had been effective on leaving the ground.

Then what has man to propose to evade these difficulties, which appear insuperable? Need I mention that he resorts to balloon propulsion in one of the various forms which are always recurring to minds who take freshly to the subject, and which are always supposed to be new.

Is there any need for me to dwell upon this negation of all hope for those who desire to navigate, and not to float and stagger about, in the air ?

There remain, therefore, the advocates of the screw-propelled plane. For the lecture-room this is a very pretty and effective experiment, and I have propelled such for years with screws both fore and aft ; but I have failed to see safety to life under such a surface, even were there not another objection, and that a fatal one, to their use.

I have always said that any aërial machine, to be safe, should have sufficient power to rise from the ground. It will then possess power to control its descent. But no plane has hitherto been devised capable of rising from the ground with any preliminary run which could be imparted to it. I have watched such experiments with much interest. Mr. Linfield's ambitious attempt failed to afford any indication of rising power, although he has travelled upon wheels at somewhere about 20 miles an hour, with a surface overhead of about 300 square feet set at an angle originally. The dimensions are—Length 40 feet, width 18 feet, height 15 feet. The weight of all, including Mr. Linfield, is 304 lbs.

Mr. Moy also tried with a model weighing 1½ lbs., also upon wheels, the screws actuated by twisted india-rubber, but he failed in my presence to effect any rise at all. He succeeded in attaining only 10 miles an hour.

The preliminary run with the velocity requisite for success seems to me to be fatal to any attempt of this nature—equally so any descent from an elevation—to attain the initial velocity.

The bird with extended wings does not rise by passively holding out his wings as a plane, but during his preliminary run he either slowly waves his wings or else tremulously vibrates them, which enables him to feel the air, and gradually to obtain a fulcrum upon it.

There is yet another group of inventors who hope, by presenting a suitable supporting surface to an accommodating breeze, to be lifted thereby, and then, by the aid of gravity and this friendly breeze, to sustain themselves like a kite, and even to progress when they get accustomed to the position.

For the last three years I have myself been a continuous experimenter, and one can scarcely work in any field of science without advancing a step in knowledge. Much more likely is the investigator to be rewarded if the field has been almost abandoned. Many a nugget has been dug out of a deserted claim. I will remark, however, in passing, that experimenters in flight are placed at a great disadvantage. The laboratory, or anything of the dimensions of a laboratory, would suffocate flight. Space and privacy are two necessities. The absence of these greatly deters progress, because for experiments in flight the object must *fly;* and we are necessarily, most of us, confined to models of small dimensions, which upon an increased scale would have more than a proportionate effect.

There is a mode of progression adopted by some fishes which is not reproduced by any flying creature, and in observing that motion—which was an undulating one—I asked myself whether in the air it would act also as a support? The fish is about the same gravity as the water; but how would the undulating motion of a loose fabric support a weight in the air? The result of my experiment was so satisfactory that it seems to me to get over the great difficulties that always suggest themselves in all plans except our own. For instance, in some designs which possessed encouraging features there seemed a want of provision for a safe and gradual descent, so that a parachute naturally suggested itself as a necessary adjunct. But the addition of a parachute means weight, and also an incumbrance. In the loose material, however, which I employ, both for support and propulsion, there *exists* the parachute, which acts upon cessation of the motive power, and brings the machine down with a gently gliding motion in the direction of its travel.

In none of the plans which have been submitted to the Aëronautical Society has there been the slightest hint that flight could be attained by imparting a wave-like action to a loose surface extended in the direction of its length; nor do I well see how it would occur to anyone to try the effect, unless he had been experimenting with flying models, so that he could readily substitute one arrangement for another. Certain it is that this discovery enables us to make a very large surface effective for support, which with wings alone appeared to be impossible. Of course, like every machine intended to find support in the air, it must be balanced; but in the model which I have made, such is its stability that an inch more or less does not affect it. So that we are at liberty to contemplate the construction of an aërial vehicle whose dimensions would suffice to maintain in wave motion six or seven hundred square feet of canvas, actuated by steam-power, and capable of supporting the additional weight of a man, whose weight, together with the machine, would certainly not exceed 500 lbs.; and we can contemplate the man as being able to move a few feet backward or forward without much affecting the stability of the machine. His descent under the parachute action can thus be graduated at will. This can also be effected by a cord attached to the tail, which by that means can be elevated or depressed at pleasure. Placed upon wheels it has, of course, yet to be ascertained what distance of preliminary run would be required, assisted by the action of the fabric, before it would rise from the ground.

In Mr. Linfield's construction I see the framework required. The alteration would be flexible arms, and I think a looser arrangement of the cloth. The treadle action might or might not be able to vibrate the wing-arms. These flexible arms I construct of a bundle of canes, about 12 or 14 feet long, bound tightly together with whipcord throughout their whole length, and cutting off a cane every 2 feet or so in order to taper them. The wing-arm then affords lightness combined with strength, for it is almost impossible to fracture it. In this case we should be spared the weight of the screw, which is something considerable.

It has to be determined by experiment what velocity and what arc of vibration is best suited to the weight to be sustained. There will be also some relation between the arc of vibration of the wing-arm and the amount of surface in undulation.

In the model which I have made there exists a strong illustration of the assertion that any device made in miniature which will fly, can, when constructed upon a scale of utility, perform not only all, but much more than all, the promises extracted from it in the model form. In the first place, the experimenter is greatly in want of some motive power which will last sufficiently long to allow of close observation. Now in the recoil of the india-rubber which I have employed to produce the revolution of the crank which vibrates the arms, there exists only power sufficient to give a dozen strokes or so. And were I to increase the throw of the crank so as to increase the arc of vibration at the end of the wing, I should require so much greater power that either I could not with my own exertions wind it up, or else the parts would fail to bear the strain. For want of that continuous power I cannot say what amount of weight the model which I have made will carry, but I know *this* to a certainty, viz., that the weight to be carried depends upon the power and the strength of material for a given surface kept in a state of vibration. The weight of the larger model is 3 lbs. 1½ ounces, of which 7 ounces is added weight, which it easily carries. The dimensions are 6 feet wide by 10 feet long, with about 16 square feet of surface. By this arrangement of loose waving surface I have had the smaller model (5 × 8) fly from my hand perfectly horizontally to the extent of 60 feet.

What has assured me that there is a necessary relation between the arc of vibration, the velocity, the weight, and the surface, is the fact that through breakage I had to review the parts, and I have never since regained that chance relationship; but the model appears to descend until the loose fabric is seen to hold the air, so that if the ballast is behind the centre of effort the surface is forced outward at an angle of perhaps 10°. Upon stoppage of the motive power there would then be no parachute action, but the machine would descend tail first. The ballast, however, properly arranged, the velocity of the model is increased, and the head being heavier, and power ceasing, onward motion continues under the concave extension of the surface.

All this would be at the command of a man with a power upon which he could depend. It is probable that if this wave-action were used in addition to a wing about a foot wide, a greater impact upon the air at starting might assist the supporting effort.

I perhaps need scarcely say that the judgment must be formed not upon the duration of flight or the distance travelled, but upon the effect observed in any part of its short flight which with my limited power I am enabled to give the model. It then becomes evident that we should only require a continuance of the power.

I think that after fourteen years' study of the subject by the members of the Aëronautical Society we are now in a far more favourable position than we have ever yet been to form a fair estimate of the probabilities of success.

The conditions necessary for flight are pretty well understood. There remain but the mechanical difficulties. How far those difficulties have now been minimised can only be satisfactorily determined by actual experiment upon a scale commensurate with the importance of the subject. And with respect to the model which illustrates this wave-action we may predicate for it that—largely constructed with power, strength, and surface—there would be as much difference in the effect as between a child's halfpenny toy, with four paper vanes pinned to the end of a stick, and the windmill whose larger vanes grind our corn.

V. BIRDS'-NESTING.

By ALLEN HARKER.

AS the time of year again comes round when the nesting operations of birds begin, it may not be unseasonable to make to our readers some plea on behalf of our native birds, and to indicate to all who may have influence to exert, or authority to exercise, on the question of birds'-nesting, methods by which alike the study of Ornithology and the interests of our bird population may be best served, and their preservation insured.

Surely of all God's creatures which delight us, those which give us the purest, most unalloyed pleasure, are the

> "Feather'd songsters of the grove"!

How much of the charm of English or Scottish landscape, of deep wooded lane and open breezy heath and common, is due to the presence of ever active, ever musical bird-life, may be best appreciated by a visit to countries where the birds are songless, or where, as in many parts of the Continent, for great stretches of country, scarce a bird is to be seen. Such a visit could not fail to impress the lover of Nature with a sense of his duty to use his endeavours to preserve from the dull monotony of a birdless country the melodious fields and hedgerows of rural England.

It is not solely on behalf of the birds themselves, and their nests and eggs, that it seems desirable to awaken greater interest and action, but also in favour of a more systematic and accurate study of the subject, combined with a true scientific use of the knowledge thus obtained—knowledge which is now in a purposeless manner allowed to become lost.

The building of a bird's nest, the choice of site, the selection and gathering of materials, the deft skill that carries on and perfects the work; the number, size, and colour of the eggs, their variations, the period of incubation, the sex of the incubator, the condition of the new-born young, their growth, and first essays at independent flight—these are events in the natural history of the bird, no less interesting or important to the ornithologist than its anatomy, its distribution, or any other of the chapters in its history. But in addition to their interest to the ornithologist, these events have ever possessed a special charm and interest for the young of all ranks of life. The beauty of the nest, and the beauty of form and colour of the egg, accompanied too by the novelty of finding such a treasure, combine to present a temptation to the finder too strong for any schoolboy to resist. The desire of possessing coloured and shining objects, which he possesses in common with the savage whose adult condition of mind he represents, leads to the destruction by him of a vast number of eggs and nests; for the youth, having taken the prize, speedily tires of his acquisition, and the eggs are soon broken, and new "sensations" sought for.

Most people will agree that such wanton destruction is to be deplored; but for ages birds'-nesting has been the schoolboy's license, and while most parents disapprove of it, few go the length of absolutely prohibiting it—still fewer use it as a means of imparting that pleasing knowledge of the natural history of birds which would soonest cure the propensity, and teach the youth at once humanity and zoology. That some efficient protection should be given to eggs and nests is the unanimous feeling of naturalists.

At present the parent birds themselves are protected during the season of nesting by a somewhat imperfect—but still, as far as it goes, a very beneficent—Act of Parliament; but it need hardly be pointed out that if a female bird is not allowed to rear any young, the chances against it (personally or by its offspring) surviving the winter, or, if it is a migratory bird, of returning in the following spring, are increased by as many times more as the number of young it might be supposed to rear; and in this way persistent and injudicious birds'-nesting may soon diminish the number of birds in a given area. And that birds'-nesting has this effect will not be doubted by any one whose study of the subject has led him to remark the diminution of small and rare birds in particular districts, or by one who has had opportunities of knowing what immense numbers of birds' eggs are annually destroyed by marauding youths.

There are always at work enough of causes—some natural, some artificial—which are not preventible, tending to diminish the numbers of our birds, more especially of our smaller song birds, either by cutting off their food supplies or by destroying their nesting grounds. Every field, marsh, or swamp that is drained, lessens the supply of insect-life on which a great majority of small birds live; every piece of land that is reclaimed from waste robs the ground or low bush nesting birds of their *habitat;* every wood that is cut down, every gorse-patch that is burned,—in short, every advance of cultivation,—drives before it some species of birds.

It was my fortune to revisit, after a lapse of ten years, a part of the country where some of my earliest birds'-nesting exploits had been carried out. "High-farming" had taken the place of a more primitive agriculture; the thick high hedges where red-backed shrikes, bullfinches, linnets, and long-tailed tits were wont to nest, were supplanted by neat trim-cut hedges 3 feet high, and not thick enough to offer cover for the smallest of birds. The deep ditches with high grass-grown banks, once the haunt of wood wren, lesser whitethroat, or whinchat, had disappeared, and patches of gorse and heather, where redpole and linnet once dwelt, had been burnt and stubbed out long ago. These causes, which for the birds' sake we may deplore, we cannot nor should we wish to prevent; and even consolation is to be found in that while one species of bird may be driven out, another suited to the new condition may follow and take its place. The richest arable land are especially the resort of the lark, who dispels the monotony with his "sweet jargoning." It is rather with preventible causes that we have to deal; and to the indiscriminate and utterly wanton birds'-nesting, for no intelligent or intellectual aim or object, which goes on in every parish in the country, a check must be applied. Here it is that the authority of parents and schoolmasters should come into force. It is in most cases due as much to *ignorance* as to wantonness or destructiveness that the youthful birds'-nester takes eggs for which he has no use—no idea of use, in fact; they serve to gratify his instinct for finding and possessing pretty objects, and then are strung on a string as an ornament, or made cockshies of as an amusing pastime. Had he been taught anything of the importance of the nest and eggs to the continuance of the parent birds,—or had any facts of their history, as of how birds differ from other animals, or how, in a sense, a nest and eggs are as much a part of the mother as the embryos of viviparous animals,—his nesting for pure wanton destruction of his spoil would at least be checked; or if he proved not amenable to such reasoning, should be forcibly prevented and heavily punished. But a remedy for checking birds'-nesting in the intelligent boy who wishes to avoid wantonness, but at the same time claims a right to make his collection of eggs as much as another collector has one to make his of butterflies or birds, must be sought in another direction, and is worth the consideration of lovers both of birds and of intelligent and inquiring schoolboys.

In passing it must be remarked that schoolboys alone are not to be blamed for purposeless birds'-nesting, and it would be unfair to pass over as great a culprit, the amateur adult collector, whose condemnation should be as much greater as is his opportunity of knowing better. It has been well remarked that few, if any, advances have been made by human beings in their history, but have been accompanied by a concomitant development of special vices, originating in a perverted application or use of the benefits gained by the advance. The form which this aberration assumes in connection with the rise and progress of biological science is as a mania for amassing large collections of animal structures, whether shells or birds' eggs, or the animals themselves, without any reference whatever to their structure or history, or to the educational purpose they might serve, when this latter exists at all. When you see in the drawers of a collector of birds' eggs a long series of the eggs of the kingfisher or a wild duck, not one egg in each series differing in any way from another, the inane purposelessness of the thing—not to use any stronger term—is evident. No clearer proof could be given that the great majority of egg-collecting—that is, by others than schoolboys—arises from a barbarous desire of possession alone, than that it is seldom if ever accompanied by the collecting of birds' *nests*, from which probably much more is to be learned of bird history than from the eggs. I need only refer to the learned observations of Pouchet on the changes which he remarked in the building of the nests of species of *Hirundo*, to instance the interesting and important results which a study of nests might lead us to.

Compare the nest of a chaffinch with those of its congeners the greenfinch or the bullfinch, or that of a sedge warbler with that of the wood wren; how totally different they are—one feels inclined to say, comparing incomparables, more different than the birds themselves. Or to go further, compare one chaffinch's nest with another, and note the variety in material, and even in construction, adapted to some peculiarity of situation or surroundings. Here is a field for observation and comparison by means of which the philosophical student may hope to catch some glimpses of the working of the laws which have taught birds, as they must in time have been taught, to build nests in endless variety of form and material. Little, if anything, has been written beyond mere speculation on this subject, which presents so much scope for investigation. In spite, too, of all the vast collections of birds' eggs which have been made, we are still almost entirely in the dark as to any theory, even, of the causes which have tended to produce such infinite variety of form and colour for what is really but for one and the same end.

The method of, at one and the same time, limiting the tendency to purposeless egg-collecting, and systematically extending our knowledge of the whole subject, which it is the purpose of this paper to suggest, is the formation in connection with local museums, or, where these do not exist, with larger schools, of complete educational collections of birds' nests and eggs. Under the judicious guidance of the head-master this might be done with but a minimum of wrong to the parent birds in at most three years, and the eggs and nests which would be required to complete such a collection would be many times fewer than what are annually destroyed in the same area, and would, by being carefully housed and attended to, obviate, as I shall endeavour to show, the necessity for repeated nesting in time to come.

Many years ago I assisted at the formation of such a collection for a small country museum, and our method of preserving and displaying it to the best advantage being devised chiefly with a view to economy of space and material, it may be useful to briefly describe it. We took our nests generally before any eggs—or but the first—had been laid, and binding them carefully with tape or cord we saturated them or sprinkled them with some preservative liquid (methylated spirits and corrosive sublimate solution is best), to kill any vermin and keep away moths in the future, and then dried them and packed them in drawers or boxes. The eggs we got when we could. Our collection being nearly complete, we had a wall case about 5 feet high and 12 inches wide, made with shelves sloping at an angle of 45°. On the bottom shelf we placed the large nests, such as those of the crow, rook, jackdaw, magpie, &c., putting into each nest the number of eggs usually laid, not necessarily, and indeed seldom, taken from that particular nest. On the second shelf, such nests as consist of little more than a few reeds or grasses, or a mere hollow scraped in earth, were contained in round card-board boxes, to hold the materials together. These included the nests of the curlew, sandpiper, gulls, coot, water-hen, ducks, grebes, &c. Smaller boxes held the collection of bones on which the kingfisher lays its eggs, the few grasses that the skylark lines a hole in the soil with, and so on. Upper shelves held the smaller nests; and when they had been built in holes of trees we cut away the branch, where possible. In time we had not only a very complete collection, but an additional case of such eggs as vary from a common type, and our little museum was the favourite resort of all the schoolboys in the district; and it is certain that much less bird-nesting for mere nesting's sake was one outcome of our efforts. Sometimes we had brought us varieties which were not found in our cases—a result which was most desirable and pleasing, as it evidenced an intelligent appreciation of the uses of a collection. A schoolmaster who will but inaugurate such a collection will certainly be conferring a lasting benefit on his pupils, and sparing many a nest from careless destruction. Certain it is, too, that if every country town and village had its small, well-ordered, local museum, where the common objects of animal and plant life—which ever will possess an absorbing interest for the young—were displayed in such a manner as to convey some intelligent ideas of their life-history and relation to each other, among the innumerable benefits which would accrue, one if not the least would be that there would be created a sympathy between the animals and their keen-eyed observers, and the wanton destruction of myriads of them would be diminished.

One of the most pleasant recollections of a short residence in a French country town is associated with its admirable museum, and the character of its visitors on Sunday afternoons. On week-days the students from an *école de medecine* shared with me the examination of its well-arranged cases; but on Sunday afternoons troops of peasants and their families took the place of the systematic student,—the husband in his clean blouse, the wife in clean starched cap of marvellous and stupendous proportions, and the children in clean Sunday best. The shouts of the youngsters at the wonders of strange foreign *bêtes* were only passed by their delight at the discovery of old friends like *M. Crapaud* or *les petites papillons bleues;* while older schoolboys conferred together over cases of beetles or bottles of reptiles, or related, in audible whispers, exploits with beast and bird which the preserved specimens recalled to mind. For two or three hours the rooms were filled with these happy-faced students of zoology; and the remembrance of Sunday afternoons nearer home, spent by British peasants in less creditable ways, rose, not unnaturally, in the mind, with the reflection that "they manage these things better in France."

In forming such a collection of birds' eggs and nests as I have described, with the desire, at the same time, to collect with such discrimination and judgment as to limit to a minimum, if not to avoid entirely, cruelty or injury to the birds, there are certain broad maxims which must be held in mind; and it is these which it is my object to strongly impress. It is well established by experience that many birds will go on laying eggs in the same nest after the loss of their first eggs; and, physiologically, there exists the most ample provision in the mother bird for such a contingency. Others will build new nests again and again after the destruction of their first efforts; but manifestly there is a limit, if only in point of time and season, to these persistent efforts at propagating their kind. No egg collector should, therefore, ever take eggs or nests after a certain date,—say, 1st of June,—except in the case of very late migrants. This limiting date should, of course, vary with different species of birds, and in different parts of the country, but should be fixed and rigidly adhered to.

A second maxim should be, that no collector should ever take partially incubated eggs, or disturb the nest in such case; and lastly, that he should never take any egg or nest at all that is not intended to form part of some new public collection, or to supply a blank in such already-established one. The common possession of a perfect collection, which might thus be speedily formed, would not only have the influences which experience has proved similar ones to have on schoolboys, but with "schoolboys of a larger growth" birds and their nests and eggs would become more familiar and interesting objects, and the ranks of ornithologists be swelled by new and devoted students. Furthermore, with such opportunities of comparing observations and discovering variations in habits of nesting, or in the phenomena connected with birds' eggs, the study of birds' nests and eggs might in time be raised to the dignity, which it scarcely at present occupies, of a science. Vast as is the number of facts which we are in possession of regarding the subject, the time scarcely seems to have arrived when these may be formulated, and a theory of birds' eggs be established. The interesting researches of Mr. Sorby into the nature and composition of the colouring-matter of birds' eggs belong rather to the sciences of chemistry and spectroscopy than to those of oology or animal physiology. What has been done towards an elucidation of the subject may form matter for future consideration. If this appeal should induce one lover of birds to take any steps to form a permanent and accessible record of their habits of nidification, it will have more than served its object.—*The Scottish Naturalist.*

NOTICES OF BOOKS.

Evolution, Old and New: or the Theories of Buffon, Dr. Erasmus Darwin, and Lamarck, as compared with that of Mr. Charles Darwin. By SAMUEL BUTLER. London: Hardwicke and Bogue.

THE work before us appears to have a threefold object. The author undertakes the rehabilitation of certain Evolutionist worthies of days bygone—possibly too much neglected and even misinterpreted—the refutation of the hypothesis of Natural Selection, as put forward by Mr. C. Darwin and by Mr. A. R. Wallace; and, lastly, the exposition of his own peculiar views on the development of species. Mr. Butler does not appear to be a working biologist, nor indeed a man of science at all. Still, unlike the bulk of outsiders who have written upon Evolution, he rejects the old hypothesis of Special Creation, and if his contentions do not everywhere command our implicit assent, they cannot fail to supply every true naturalist with abundant matter for profitable reflection.

It is generally, and perhaps too hastily, assumed that the Doctrine of Development, as met with in the works of Buffon, Erasmus Darwin, and Lamarck, was so crude and imperfect as to necessitate its rejection by men of sound and sober judgment, whilst Mr. C. Darwin, by supplying his idea of Natural Selection, has converted the chaos into a cosmos, and rendered the adoption of Evolutionism not merely possible, but almost imperative. Mr. Butler, on the contrary, holds the very contrary opinion:—"Fresh from the study of the older men, and also of Mr. Darwin himself, I failed to see that Mr. Darwin had 'unravelled and illuminated' a tangled skein, but believed him, on the contrary, to have tangled and obscured what his predecessors had made in great part, if not wholly, plain. The older men, if not in full daylight, at any rate saw in what quarter of the sky the dawn was breaking, and were looking steadily towards it." Again we read—"Those were the days before 'Natural Selection' had been discharged into the waters of the Evolution controversy, like the secretion of a cuttle-fish." One of Mr. Butler's charges against "Darwinism" is that, shutting out purpose and intelligence from the work, it refers the formation of species to the accumulation of small and hap-hazard variations—an objection which has been urged by Mr. J. J. Murphy and by Mr. Mivart. A not less capital point is that Natural Selection, though it may preserve any variation that proves advantageous, has plainly nothing whatever to do with the origin of such variation. This will at once appear if we reject the words "Natural Selection," and write in their stead the expression "Survival of the Fittest," which Mr. Darwin himself declares to be equivalent in meaning. Everyone will then admit that before the fittest can survive it must come into existence. Here Mr. Darwin leaves us in the dark, giving us an "Origin of Species" with the "Origin" cut out. But this is not all: the bulk of the readers of Mr. Darwin's work go away with the impression that Natural Selection is the most important cause of modification. Now Mr. Darwin certainly protests against being understood to assert that Natural Selection induces variability, and yet, perhaps from inadvertence, uses language open to a very different interpretation. Thus he declares Natural Selection the most important "means" of modification. Here Mr. Butler remarks—"'Means' is a dangerous word; it slips too easily into cause. We have seen Mr. Darwin himself say that Buffon did not enter on '*the causes or means*' of modification, as though the two words were synonymous or nearly so. The use of the word 'means' enables Mr. Darwin to speak of Natural Selection as if it were an active cause (which he constantly does), and yet to avoid expressly maintaining that it is a cause of modification."

It will now be asked, what does Mr. Butler offer us instead of Natural Selection as a more satisfactory solution of the mutation of forms and the rise of species. Taking up the views of Lamarck and of the elder Darwin, he contends that "differentiations of structure and instinct are due to the desires under changing circumstances of an organism, which must be regarded as a single creature, though its development has extended over millions of years."

Between parents and offspring there is oneness of personality —a view which explains the phenomena of heredity and of instinct, or, as it might be called, unconscious memory. Purpose and intelligence are thus brought into play in the genesis of species, but the intelligence is not that of an Allwise Creator, which cannot be supposed as taking tentative steps, or abandoning its plans as unsatisfactory and working in a different direction.

With reference to the nidification of birds, we find a case quoted from Miss Seward's biography of Erasmus Darwin which supplies the crucial experiment asked for by Mr. Wallace in his Essay on the "Philosophy of Birds' Nests."* A pair of young canaries, who had never seen a nest built, are described as constructing their nest on the slovenly regulation pattern of their race, "even to the precise disposal of every hair and shred of wool." For such a fact Mr. Butler's theory accounts without the invocation of a mysterious "instinct."

Into the biographies of Buffon, Erasmus Darwin, and Lamarck, and into the author's exposition of their views, space does not allow us to enter. We can only quote two passages, very different in their scope, but equally suggestive :—" Science is not a kingdom into which a poor man can enter easily if he happens to differ from a philosopher who gives good dinners, and has 'his sisters and his cousins and his aunts' to play the part of chorus to him."

---

* Contributions to the Theory of Natural Selection, p. 211.

"For our skull is as a kind of flower-pot, and holds the soi from which we spring,—that is to say, the brain ; our mouth and stomach are roots in two stories or stages ; our bones are the trellis-work to which we cling ; *we* are the nerves which are rooted in the brain, and draw thence the sustenance supplied by the stomach ; our lungs are leaves folded up within us, as the blossom of a fig is hidden within the fruit itself." We are thus likened to " perambulating vegetables turned upside down."

"Evolution, Old and New " is well worth reading."

A Manual of Organic Chemistry, Practical and Theoretical. For Colleges and Schools, Medical and Civil Service Examinations, and especially for Elementary, Advanced, and Honours Students at the Classes of the Science and Art Department, South Kensington. By HUGH CLEMENTS, of H.M. Civil Service ; President of the Amateur Mechanics' Workshops Association, London ; and Lecturer on Various Sciences at St. Thomas Charterhouse and several other Science Institutions in London. London : Blackie and Son.

THIS Manual is in great part a reprint from the " English Mechanic," and is unquestionably an examinational work. It contains chapters on the distinction between organic and inorganic substances ; on organic analysis and empirical formula (*sic*) ; on the determination of rational formulæ ; on organic substitution ; on the theory of compound organic radicals ; the preparation and properties of their hydrides ; the distillation of coal ; on alcohols, ethers, aldehyds, organic acids, anhydrides, ketons, alkaloids, and organo-metallic compounds. Then follows a section on the identification of organic compounds ; and a description of oils, fixed and essential. We have next an account of certain articles of apparatus, a selection of exercises, and a list of "Papers set in Organic Chemistry at the Examinations of the Science and Art Department from 1868 to 1878, with Answers," —a section which occupies nearly one-third of the entire book, and which may give the student, or rather the intending examinee, some idea of the class of questions likely to fall to his lot.

That the bulk of the matter contained in the work is on a level with the knowledge of the day is unquestionable. Still we come here and there upon passages which, to us at least, seem to require emendation. Thus the author tells us that as carbonic anhydride "is a product of the respiration of animals, it is decidedly an organic product, and should therefore be included with organic compounds"—an argument which we can scarcely admit. Blood receives a formula $C_{45}H_{40}N_6O_{15}$, as though it were a definite chemical individual. Yeast, we learn, "uninfluences it" (*i.e.*, gum). Human bile "has a nauseous fragrant odour."

On p. 50 we have, *verbatim et literatim*, the following passage:—"Picric or trinitrophenylic acid, like any acid, forms salts that crystallise well. It has the odour of tar or creosote, and has a hot taste. It is much used as a disinfectant, rendering sewage, &c., inoffensive. It quickly arrests all fermentative and putrefactive changes, &c." Now the chemist who knows the properties of picric acid will here call a halt, and, looking back, perceive that what is said concerning disinfecting and antiputrescent properties must refer not to picric acid, but to phenol, of which the author has been previously speaking. But the student who may have no previous knowledge of picric acid and of phenol, and who can merely follow the grammatical construction of the passage, will here, we submit, be led somewhat astray.

Speaking of fats Mr. Clements says—"The fat of warm-blooded animals is generally solid, while that of cold-blooded animals *and* fish is liquid." Fish are generally supposed to be "cold-blooded animals," but this sentence shows that our "lecturer on various sciences" is of a different opinion. He continues—"The principal solid fats are butter, grease, lard, and suet, and the fluid animal fats are cod-liver, neats' foot, sperm and whale oils." It is curious that, of the fluid fats enumerated, all, save one, should be the products of warm-blooded animals.

These peculiarities, which are not the only ones that might be selected on close scrutiny, show that the "Manual of Organic Chemistry" needs revision before it can be pronounced a trustworthy guide.

Bulletin of the United States Geological and Geographical Survey of the Territories. Vol. iv., No. 4, and Vol. v., No. 1. Washington: Government Printing-Office.

THE first of these two numbers contains an account of the fossil insects found in the Tertiary shales of the Green River, Wyoming, by that well-known entomologist S. H. Scudder. The collection, eighty species of which have been identified, is remarkable for the distinctly tropical aspect of its forms. Dr. D. S. Jordan reports on the collection of fishes made in Dakota and Montana by the indefatigable Dr. Elliott Coues; whilst Prof. J. W. Chickering furnishes a catalogue of phænogamous and vascular cryptogamous plants collected by the same naturalist in the above districts. Mr. F. M. Endlich's paper on "Erosion-products in Colorado" would prove a most instructive study for those—and such writers exist—who deny that erosion has any share in the formation of the earth's surface. Dr. C. A. White discusses the palæontology of the "Laramic group," and Mr. J. A. Allen gives a synonymatic list of the American *Sciuri*, or tree-squirrels.

The first number of Vol. v. contains much interesting matter. Dr. C. V. Riley and Mr. J. Monell contribute a monograph of the American Aphididæ—a group of insects of loathsome aspect, but interesting from their peculiar system of reproduction, and from the ravages they commit in the fields and gardens. The first annual generation of *Schinoneura Americana*, the American elm Aphis, consisting entirely of females, issues from impregnated eggs which have survived the winter, and without sexual intercourse produces a second generation like themselves in structure. The third generation is winged. The fourth and fifth again are wingless, the sixth winged, whilst the seventh consists for the first time of individuals of both sexes. Each female deposits a single egg, which is not hatched till the ensuing spring. Amongst the natural enemies of this species the author enumerates several Coleoptera, especially Coccinellidæ, two Hemipterous species, and even the larva of a moth (*Semasia prunivora*). The misfortune is that the enemies of Aphides are, in point of number and voracity, utterly inadequate to the task set before them.

Prof. E. D. Cope discusses the relations of the horizons of extinct Vertebrates of Europe and North America. The author points out, as a consequence of the principle of descent, that the types of each age have taken their origin from the generalised types of preceding ages, there being no descent from the more specialised types. Hence in discriminating the subdivisions of geological time we have to be guided by the disappearance rather than by the appearance of species. He finds that portions of all the faunæ of all the primary divisions of geologic time have been recognised on both continents,—that parallels requiring general identification of the principal divisions of these faunæ may be detected, but that exact identifications of restricted divisions may be made in a few instances only. Between the tables of extinct animal and vegetable life there is a discrepancy. The plant-life of North America reached its present condition one epoch earlier than did the higher Vertebrata. Hence either the animal-life of North America has lagged behind that of Europe, or the flora of America has been in advance of the European. The author mentions that the earliest land vertebrates had a persistent *chorda dorsalis*.

This number further contains papers on the miocene fauna of Oregon; on the birds of Dakota; on the fossils of the Jurassic trias of south-eastern Idaho and western Wyoming; on the fossilised forests of the volcanic tertiaries in the Yellowstone

National Park; on the conditions of preservation of invertebrate fossils; and a supplement to the bibliography of North American invertebrate palæontology.

The Journal of the Cincinnati Society of Natural History. January, 1879. Vol. i., No. 4.

In this number we have a paper by Mr. V. T. Chambers on the tongue of certain Hymenoptera. The tube in this organ is in the hive-bee 1-500th of an inch in diameter, but in some of the Andrenidæ only half that width and closed at the apex. It is curious that though Fritz Müller and Wolff, as well as the author, have demonstrated the tubular character of a bee's tongue, it should be described as solid in the latest edition of the "Encyclopædia Britannica."

The most important article, however, is a very elaborate list of the birds occurring in the Cincinnati district. We shall not be surprised if, in spite of the "British" mania prevalent among ornithologists, entomologists, and botanists in this country,—a whim which leads to little save the extirpation of our rarer forms, the range of each native species will be mapped out accurately in America sooner than in these islands.

We learn, with little surprise, that the introduction of the European sparrow into the United States is now looked upon by competent authorities as a deplorable mistake. We hope that other parts of the world, not yet suffering from the presence of this "winged rat," will take warning from the experience of our American friends. Acclimatisation is not a matter to be rashly undertaken.

Euclid and his Modern Rivals. By Charles L. Dodgson, M.A., Senior Student and Mathematical Lecturer of Christ Church, Oxford. London: Macmillan. 1879.

Mr. Dodgson's drama, or rather dialogue, opens with a soliloquy from "*Minos*," an examiner tortured by having to look over papers in Geometry, containing a mixture of the old and new systems. Minos, after some conversation with "*Rhadamanthus*," another equally unhappy examiner, falls asleep, and to him appears the shade of Euclid ready to do battle for his Manual. A discussion follows, in which the reasons for *retaining* Euclid's Manual are treated, but it is noticeable that those for rejecting it are only just mentioned; and even that is denied to many.

In fact, throughout his book, the author is far more inclined to attack Euclid's Rivals than to defend Euclid himself, so that a one-sided view of the question is shown, and Euclid's faults are kept steadily in the back-ground. Euclid and Minos then discuss the method of procedure in examining Modern Rivals, and a few alterations proposed by them.

In the Second Act Minos is discovered still sleeping, and to him enters a shade which is addressed as "*Niemand*," with copies of the Modern Rivals to be discussed. These are divided into two classes—those which reject Euclid's treatment of parallels, including Legendre, Cooley, Cuthbertson, Wilson, Pierce, and Willock; and those which adopt Euclid's treatment of parallels—these are Chauvenet, Loomis, Morell, Reynolds, Wright, the Syllabus of the Association for the Improvement of Geometrical Teaching, and Wilson's Manual founded on that Syllabus.

In the Fourth Act the shade of Euclid again converses with Minos, and, with the exception of some one or two slight additions and alterations, such as a proof that all right angles are equal, it is decided that Euclid's Manual ought to be left untouched.

The chief points of attack on Euclid's Modern Rivals are Mr. Wilson's two works—"Elementary Geometry" and the "Manual founded on the Association Syllabus." The author makes comparatively short work of Legendre's book as unsuited to beginners, though doubtless valuable to advanced students; and of Cooley's, in which a certain theorem breaks down through a faulty definition of parallel lines. But to Mr. Wilson's two Manuals he devotes nearly a third of his volume. Much of the criticism on these, however, is mere cavilling; for instance, at page 177, Minos says, speaking of Wilson's "Syllabus" Manual:—"At p. 57 I see an 'Exercise' (No. 5). '*Show that the angles of an equiangular triangle are equal to two-thirds of a right angle.*' In this attempt I feel sure I should fail. In early life I was taught to believe them equal to *two right angles*—an antiquated prejudice no doubt; but it is difficult to eradicate these childish instincts." This is mere straw-splitting; *strictest* accuracy would of course require the insertion of "*each*" before "equal," but if the sum of the interior angles had been intended to be understood "*together*" before "equal" would have been absolutely necessary. The very next paragraph, containing an accusation of the fallacy *Petitio Principii*, is another instance of cavilling criticism; for to take a line greater than half another, surely we need only take it greater than the whole, and that involves no knowledge of the bisecting point of the line.

At page 160 there is a criticism on the definition of a right angle as given by the Association for the Improvement of Geometrical Teaching in their Syllabus. This is—"When one straight line stands upon another straight line, and makes the adjacent angles equal, each of the angles is called a right angle."

Since the Association Syllabus admits of angles equal to or greater than two right angles, this is open to the objection that it does not debar the case in which one line stands on the *end* of the other, making the adjacent angles equal to one another, and to *two* right angles as right angles are generally considered. That is certainly a grave objection, but the same applies equally to Euclid's definition, or else a proof must be supplied, which is not that in the case mentioned the two lines are in one and the same straight line; and so this interpretation is debarred by Euclid's limiting clause.

In fact, though Mr. Dodgson's book is interesting and often witty, he fails to prove his point, because he takes a one-sided view of the question, and merely exhibits the blunders of Euclid's Rivals without balancing them against Euclid's own. Besides which—though perhaps it is more readable than an essay—a dialogue does not seem to be the clearest form for setting forth arguments and facts. It is certainly an advantage that the more solid part of a treatise of this sort should be enlivened here and there by lighter matter, more especially as it is so managed as not to break the thread of the argument.

The Tables of Problems—though there is a somewhat complex system of notation to be mastered before they become intelligible—are extremely interesting; more especially Table II., which is described as "consisting of theorems admitted to be real and valid, but *not* deducible from undisputed axioms."

CORRESPONDENCE.

THE SENSES.

To the Editor of the Monthly Journal of Science.

Sir,—In an article which you inserted not very long ago the possible existence of other senses than the five with which we are personally familiar was suggested. You will perhaps therefore permit me to call your attention to a lecture by Professor Pierce, of Cambridge (U.S.). He points out that auditory vibrations are not more than twenty thousand per second, whilst the slowest visual vibrations are four hundred millions of millions. Between these two limits, therefore, there is a range sufficient for more than forty additional senses, "each of which might have its own peculiar effect upon the nerves of the observer, and give a corresponding variety of information." Surely this consideration alone ought to convince us of the vast extent of our own ignorance and of the folly of pronouncing on the non-existence of whatever escapes our scanty means of research.—I am, &c.,

Caution.

HEREDITY.

To the Editor of the Monthly Journal of Science.

Sir,—A recent writer explains the heredity of talents, character, and instincts by the hypothesis that the young animal is a continuation of its parents, and enters upon life with a remembrance of their actions, which, though latent, is roused into activity by the force of circumstances. Hence the young bird, when the breeding-season comes on, builds a nest on the pattern followed by its ancestors. This assumption offers an excellent explanation of the peculiar type of nest adhered to by each species, and even of the architectural peculiarities of the solitary wasps, bees, &c., who have never even seen their parents. But I find a difficulty as regards the song of birds. Experience shows that a young bird brought up without the society of its own species will never adopt their characteristic note, any more than a child reared among wild beasts—of which there have been cases—will speak the language of its parents. Now if a bird is a continuation of the personality of its ancestors, why should it not adopt their song as instinctively as their style of nest-building.—I am, &c.,

AN OLD NATURALIST.

THE BAND PATTERN IN ANIMALS.

To the Editor of the Monthly Journal of Science.

SIR,—In reply to J. W. S. permit me to say that the facts he mentions respecting the band pattern in animals have often been observed. In a recent number of "Science for All" (vol. i., p. 252), Mr. W. Ackroyd remarks that a bilateral symmetry of marking exists in the head of the Bengal tiger and bodies of zebra, the Indian tapir, the Aard wolf, and some of our domestic cats. He then proceeds to adduce many facts which seem to point out that colour uniformities—*i.e.*, sexual differences, bilateral symmetry, and colour distribution generally—are "regulated by some deeply-seated and symmetrically distributed portion of the organism, such as the nervous system.

I may observe that the bands are often parallel to the axial line, there being many examples of this in the British Museum. May we not, therefore, look upon the band pattern as of three kinds with respect to the axial line:—1st, the right-angle pattern; 2nd, the parallel pattern; and 3rd, what may be taken as a mixture of the two—the spotted pattern?—I am, &c.,

L. J. DE WHALLEY.

June 23, 1879.

PROCEEDINGS OF SOCIETIES.

ROYAL SOCIETY, *May* 1.—"On the Origin of the Parallel Roads of Lochaber, and their bearing on other Phenomena of the Glacial Period," by Joseph Prestwich, M.A., F.R.S., F.G.S., &c., Professor of Geology in the University of Oxford. Of the various hypotheses that have been brought forward since the time of Macculloch and Dick-Lauder to account for the origin of the parallel roads of Glen Roy, the one so ably propounded by Mr. Jamieson, in 1863, has been most generally received and adopted. It is a modification of the views originally expressed by Agassiz, to the effect that the barriers of the lakes—to the shore action of which both the above named geologists attributed the "roads," but were at a loss to account both for the formation and removal of barriers—had been formed during the Glacial period by glaciers issued from Glen Treig and Glen Arkaig, supplemented by others from Ben Nevis. The subsequent determination, by the Scotch geologists, of an intermediate milder period succeeded by a second cold period, led Mr. Jamieson to conclude that the extension of these two glaciers took place during the second cold period, which he thinks was of little less intensity than the first, and that, while the glacier from Glen Arkaig blocked up Glen Gluoy, the glacier from Glen Teig formed a barrier to Glen Roy. This and other objections are considered by the author to be fatal to the hypothesis advanced by Mr. Jamieson; but while objecting to this exposition of the Glacial theory, he considers that that theory affords the most satisfactory solution of the problem, only that he would suggest a different interpretation in explanation of the phenomena. Dismissing the hypothesis of local glaciers of the second period of glaciation, the author falls back upon the original idea of Agassiz with the development acquired by more recent research, and assigns the Lochaber lakes to the close of the first period of great glaciation. The general conclusions drawn by the author from the phenomena in Lochaber and surrounding district are—

1st. That at the period of the first great glaciation of Scotland the ice-sheet in Lochaber attained a thickness of not less than 2000 to 2500 feet, but that in consequence of the peculiar physiographical conditions of the district the large ice-currents from Ben Nevis so clashed with others in the Spean and Lochy Valleys that a block ensued which led to an exceptional heaping up and accumulation of the ice in front of Glen Roy and Glen Gluoy.

2nd. In consequence of the lowering and partial submergence of the land, and its conversion from a continental area to an archipelago, combined possibly with some other more general cause, an amelioration of the climate took place, attended by a gradual melting of the ice-sheet in all the lower tracts. The snow and ice wasted from the valleys and from the lower mountain summits, and, in the absence of any established watercourses, the hollows and depressions in the ice, when not fissured, were converted into pools and tarns, until the continued liquefaction opened out surface channels or interior fissures, by which the water could ultimately escape.

3rd. That pending the establishment of natural lines of drainage, and in presence at places of unusual obstruction, the water accumulated in some valleys in larger bodies or lakes; and if in those cases, the mouth of the valleys being closed by the main ice-barriers, other channels of escape—such as cols or passes—communicating with adjacent glens or valleys presented themselves, the water overflowed through these channels as soon as it rose to the height of such cols or passes. Or should the cols have been also barred by ice, that ice would have given way as soon as the increasing height and pressure of the water proved sufficient. When this happened the water would at once fall to the fixed level regulated by the col, and thus no record, such as we have in Lochaber, might be left of the presence of the original bodies of water.

4th. In the Lochaber district, while the exceptional accumulation of ice in the Spean Valley barred the entrance of the glens on the north side of that valley, their passes were also blocked by remnants of the great ice-sheet; and the formation of the detrital shelves is due to the sudden bursting of these minor barriers, when the waters of the lake were discharged with great rapidity, until they fell to the level of the col. Under these circumstances the mass of loose *débris* covering the hill sides below the line of water-level gave way, and slid atter the retreating waters, until stayed with greater or lesser abruptness, according to the angle of slope and the volume of the mass, on the discharge ceasing and the waters coming to rest. The shelves so formed, modified slightly by subsequent subaërial action, constitute the "roads."

5th. The moraine detritus in places where the glaciers clashed, and where their progress consequently became checked or delayed, tended also to accumulate or heap up, and in this way in the Lochaber glens added to the strength and permanence of the ice-barriers.

6th. While the moraine detritus was irregularly distributed under the ice, or massed in particular places, the *débris* projected on the surface of the ice-stream, and contained in its body, was either left *in situ* on its liquefaction or else was—as the result of the great floods consequent upon the bursting of lake barriers—

carried successively to lower levels, leaving here and there banks of sand and gravel at various heights on the hill sides. These destructive floods, combined with the incessant river inundations due to the same general thaw of the great ice-sheet, carried down and spread out in the valleys and plains the great beds of gravel and sand which—with the modifications since brought about by long-continued fluviatile action—have given rise to various forms of escars, terraces, and other less-defined accumulations of these detrital materials.

May 15.—"Some Researches with Professor Hughes's new Instrument for the Measurement of Hearing—the Audiometer," by Benjamin Ward Richardson, M.D., LL.D., F.R.S.* The Audiometer consists of two Leclanché cells for the battery, a new and simple microphonic key connected with the cells and with two fixed primary coils, and a secondary or induction-coil the terminals of which are attached to a telephone. The induction-coil moves on a bar between the two fixed coils, and the bar is graduated into 200 parts, by which the readings of sound are taken. The graduated scale is divided into 20 centims., and each of these parts is subdivided into 10, so that the hearing may be tested from the maximum of 200 units to 0° —*zero*. The fixed coil on the right hand contains 6 metres of wire; the fixed coil on the left hand contains 100 metres. By this means a long scale from the left-hand coil is produced. The secondary coil contains 100 metres of wire. In using the instrument, one Leclanché cell has been found sufficient, as a general rule; but two have been used in instances where the hearing of the person under test has been very defective. The person whose hearing is being tested should sit in an easy position, and should not see the act of the observer in moving the microphone key. For good observation the room in which the experiment is made should be large, and all external causes of sound—such as the ticking of clocks, the vibrations of windows and doors, the moving of feet, and the singing from gas jets—should be silenced. The sitter should close the ear that is not applied to the telephone while he is listening for minute sounds, and should give his full and calm attention to the proceeding. The instrument may be considered to afford the most satisfactory means for testing the hearing power of all persons who can define a sound. The range of sound is sufficient at the maximum, 200°, for everyone who is not absolutely deaf 0°, or *zero*, is a point of positive silence from the instrument, or rather from the sound which it produces through the telephone. One of the first facts learned with the audiometer is the suddenness with which the sound is lost to those who are listening. The sound is abruptly lost within a range of 2°; that is, within one-hundredth part of the entire scale. This is the case with those who are very deaf, as well as with those who hear readily. The effect of filling the chest and holding the breath makes a difference in listeners. The capacity for hearing is for a few seconds increased by holding the breath. Holding the breath with the chest not full fails to produce the same result. As a rule, the hearing of persons who are right-handed is most refined in the right ear, and as most persons are right-handed it is found that the right ear is the best ear. This rule is, however, attended with many exceptions, since, for various reasons, some persons who use the right hand exclusively, practise for some particular purpose the use of the left ear, upon which that ear becomes more acute. Connected with the last-named fact is another, namely, that by this instrument the deaf are found to fail in capacity of hearing not only by reason of physical defect, but also by failure of memory of sounds. Thus in a youth who had suffered serious defect of hearing for seven years, owing to partial destruction of the tympanum, and who in the right ear could only detect sound at 107°, there was an inability to catch all the sound lying between 130° and 107°, until he could remember what he had to listen for. By practising him then to detect the lowest sound that he was physically capable of receiving, the author got him to detect this one sound more readily than those which came higher up. By further practice all the intervening sounds became audible with equal facility. These facts, which have been confirmed by another observation on a different person, seem to indicate that deafness from imperfection of the tympanum or other parts of the organ of hearing may be increased, beyond the mere physical failure, either from some lost power of automatic adjustment in the auditory apparatus or from failure of receptive power in the cerebrum itself, so that the memory rendered imperfect is slow to assist the listener until by exercise of function its readiness is restored. By use of the audiometer the influence of atmospheric pressure on hearing is detectable. In the author's own case, when the barometer is at 30° he can hear on both sides close down to zero, but below 30° he fails by 2° on the left side to reach zero. Dr. Richardson has tried to determine in some of the lower animals whether there is the same sense of hearing as in man. In most animals it is difficult to obtain sufficient quietude to enable the observer to gather from expression or movement of the animal the information sought for. In two dogs—one a terrier, the other a field spaniel—he succeeded in making some good observations, and in them the range of hearing power seemed to be distinctly lower than it is in the human subject who has perfect hearing. In both these animals, which were healthy and in the prime of life, the first indication of the detection of sound commenced at 10° on the scale. The detection was evidenced by the sudden expression of listening, by a slight change of position, and a slight dilatation of the pupils. This detection was clearly made on the instant, as if the sharp line of hearing were the same in them as it is in the higher animal.

---

* **For a description of Prof. Hughes's induction-currents balance see p. 508.**

The following practical memoranda are given by the author:—

"1. The audiometer will, I think, be an essential in all physical examinations of men who are undergoing examination as to their fitness for special services requiring perfect hearing, such as soldiers, sentries, railway officials, and the like.

"2. The instrument will be of great use to the physician in determining the value of hearing in those who are deaf, and in determining the relative values of the two organs of hearing. In one instance, already, I have been able by its means to detect in a person who was supposed to be equally deaf on both sides, that on one side the hearing is perfect close up to zero, while on the other side nine-tenths of the hearing is lost.

"3. In other forms of diagnosis I have found the instrument useful. In a young person suffering from acute anæmia the hearing was so defective that on the right side it failed to detect sound at 18°, and on the left side at 15°. In ten days, during which, under a new regimen, great improvement took place in strength and general condition, the power of hearing had so much improved that the right ear was good down to 12°, and the left to 3°,—an improvement of 6° on the right, and of 12° on the left side. In another person, who was subject to repeated vertigo, the giddiness occurring three or four times a-day, the hearing was so defective that although the external ear on each side was clear and the tympanum natural, no sound could be heard below 30°. Under complete rest and attention to diet the vertiginous attacks were in a few days removed altogether, and with that removal there was gain of hearing, on both sides equally, up to 5° on the audiometric scale.

"4. The instrument may be used to differentiate between deafness through the external ear and deafness from closure of the Eustachian tube—throat deafness. In my own case I fail to detect sound by the mouth at 170°, and this I find is a fair average in those who are healthy. It represents the comparative value of communication by sound through the Eustachian canal and the external ear.

"5. The instrument promises to be very useful in detecting the effects in the body of those agents which quicken or excite the circulation, such as alcohol and other similar chemical substances.

"6. The instrument promises to be of great service in determining the value of artificial tympanums in instances of deafness due to imperfection or destruction of the natural tympanum. The cotton artificial tympanums introduced originally by the late Dr. Yearsley, and the membranous tympanums introduced by the late Mr. Toynbee, F.R.S., have proved of much service; and by means of the audiometer I have been able very accurately to test their respective merits, and to compare both with tympanums made of other material. The inquiry has led me to test different metals for this purpose, and to find in fine gold the substance for making the most useful and effective artificial drum."

"Note on the Invention of a Method for making the Movements of the Pulse audible by the Telephone: the Sphygmophone," by Benjamin Ward Richardson, M.D., LL.D., F.R.S. While experimenting with the audiometer it occurred to me that I might get a secondary or telephonic sound from the movements of the pulse at the wrist. I have effected this in a very simple manner, by adding a microphone to a Pond's sphygmograph. I mount on a slip of talc a thin plate of platinum. I place the talc slip in the sphygmograph as if about to take a tracing of the pulse. I connect one terminal from a Leclanché cell to the slip of platinum on the talc, and the second terminal from the cell to a terminal of the telephone. Then I connect the other terminal of the telephone with the metal rod of the sphygmograph which supports the talc. The instrument is now ready for use. It is placed on the pulse, in the ordinary way, and is adjusted, with the writing needle thrown back, until a good pulsating movement of the needle is secured. When the movement is in full action the needle is thrown over to touch the platinum plate, which it traverses with each pulse-movement, and completes the connexion between the telephone and the battery. The needle, in passing over the metallic plate, causes a distinct series of sounds from the telephone, which correspond with the movements of the pulse. When all is neatly adjusted the sounds heard are three in number, one long sound and two short, corresponding to the systolic push, the arterial recoil, and the valvular check. The sounds are singular, as resembling the two words "bother it." The sounds can be made very loud by increasing the battery power. This little instrument is not so good a recorder of the pulse as the sphygmograph, but it may be made very useful in class, for illustrating to a large number of students, at one time, the movements of the natural pulse and the variations which occur in disease. I call the invention the Sphygmophone.

"Note on a recent Communication by Messrs. Liveing and Dewar," by J. Norman Lockyer, F.R.S. In a paper of last December the author called attention to the importance of discussing Young's observations of the chromospheric lines in connexion with the spectra of the metallic elements. Since his paper was read Messrs. Liveing and Dewar have given a table which professes to state the number of times various lines in certain metals were seen by Young in connexion with certain reversal phenomena observed by themselves. The statements, however, made in this table with regard to the visibility of certain lines in the chromosphere do not appear to the author to be in accordance with Young's published tables, and he considers that a higher degree of accuracy than that employed by Messrs. Liveing and Dewar is necessary to determine such coincidences.

The author's eleven years' work on this special branch has led him to the conclusion that all statements of coincidences between metallic and solar lines with a lower degree of accuracy than that employed by Thalén and Young are to be avoided when possible, as they may be worse than useless—they may mislead. Although the map on which he was working was on twelve times the scale of Angström's, it would have been better if it were larger; and in saying this he adds his tribute of admiration of the accuracy of the work of those who have preceded him, notably Angström, Thalén, Cornu, and Young, with whose work he is more familiar, as it is expressed in wave-lengths.

May 24.—"Note on the Spectrum of Sodium," by J. N. Lockyer, F.R.S. I have lately been engaged in studying the spectrum of sodium under new experimental conditions. In anticipation of a detailed communication I take leave to state that the vapour given off from the metal after slow distillation in a vacuum for some time shows the red and green lines without any trace whatever of the yellow one. Hydrogen is given off in large quantities, and at times the C line and the red "structure" are seen alone. After this treatment the metal, even when red-hot, volatilises with great difficulty.

May 29.—"Researches on Explosives. No. II. (Fired Gunpowder)," by Captain Noble, late R.A., F.R.S., F.R.A.S., F.C.S., and F. A. Abel, C.B., F.R.S., V.-P.C.S. The authors preface this memoir by two tables: one of these gives the results of some analyses of products of explosion of the three services powders of Waltham Abbey manufacture—pebble, R.L.G., and F.G.—which are required to complete the series of results obtained by firing the charge in different spaces, which were given in their first memoir on this subject, and a statement of the mean percentage composition by volume of the gases and by weight of the solid products furnished by those three powders, together with the highest and lowest proportions in which each product has been obtained with the three descriptions of powder. This table includes the results of examination of the products of explosion of four descriptions of powder differing in many respects from the powders chiefly employed in their researches. The other table gives the complete results of the whole series of analyses made, showing the proportion by weight of each solid and gaseous product, and including the amount of water pre-existent in the various specimens operated on. A portion of this memoir is devoted to a discussion of a few points raised in reference to the first memoir of the authors,* on fired gunpowder, by General Morin and M. Berthelot, who were appointed by the Académie des Sciences as a Commission to report on that memoir. In discussing M. Berthelot's views respecting the objections which the authors raise against the acceptance of any chemical equation as giving even a general idea of the metamorphosis which a gunpowder of average composition may be considered to undergo when exploded in a confined space, they disclaim having had any intention to convey the impression, which indeed they consider that their expressions were not calculated to convey, that it was impossible to put into *some* form of equation a representation of a variety of reactions which, if assumed to take place simultaneously, among different proportions of the powder-constituents, might give approximate expression to the results obtained in any one particular experiment, and might, thus far, afford some approach to a theoretical representation of the metamorphosis of gunpowder. What they desired to point out and lay stress upon was the conclusive proof, which is afforded by the very great variations in composition, of the solid portions more particularly, of the products of explosion of samples of gunpowder presenting only small differences of composition (and even of the products furnished at different times by one and the same sample), that the reactions which occur among the powder-constituents are susceptible of very considerable variations, regarding the causes of which it appears only possible to form conjectures, and that, consequently, " no value whatever can be attached to any attempt to give a general chemical expression to the metamorphosis of gunpowder of normal composition." An examination of the results of the author's experiments on the heat generated by the combustion of gunpowder shows, first, that the heat thus generated is subject to very wide variarions, dependent upon the particular nature of the powder employed (the Spanish powder, for example, generates just 50 per cent more heat than the mining powder); and, secondly, that the heat evolved by the same description of powder varies in different experiments to a greater extent than is to be accounted for by errors of observation. The authors' views on this head are confirmed by calorimetric determinations in their researches on gun-cotton. In these determinations, which have been carried on with precisely the same apparatus, no appreciable difference was found in the heat evolved in the various experiments. It is of high importance to observe that the volume of the permanent gases generated is in every case in inverse ratio to the units of heat evolved, as is shown by the following table .—

---

* Phil. Trans., 1875, Part 1.

Nature of Powder.	Units of Heat per Gramme exploded.	Cub. centims. of Gas per Gramme exploded.
Spanish pellet powder ...	767·3	234·2
Curtis and Harvey's No. 6	764·4	241·0
W.A.F.G.	738·3	263·1
W.A.R.L.G.	725·7	274·2
W.A. Pebble	721·4	278·3
Mining	516·8	360·3

The results given in this table are very striking. Taking the two natures of powder which commence and close the list, the heat generated by the Spanish powder is about 50 per cent higher than that generated by the mining powder, while the quantity of permanent gases evolved by the latter is about 50 per cent greater than that given off by the former. Thus it appears that the great inferiority of heat developed by the mining powder, as compared with the Spanish powder, is compensated, or at least approximately so, by the great superiority in volume of permanent gases produced. A similar relation is observed in respect to the other powders, and it would indeed appear that the pressures at any given density and the capacity for performing work of the various powders are not very materially different. This fact has been entirely verified for the whole of the Waltham Abbey powders, and in a less degree for the three other powders also. The peculiarities shown by the mining powder are so interesting that it appeared important to determine its tension when fired under a high gravimetric density. 11,560 grs. (749 grms.) of this powder have therefore been fired under a gravimetric density of unity. The pressure developed by two very accordant observations was, when corrected, 44 tons on the square inch (6706 atmospheres). The pressure obtained under similar circumstances from Waltham Abbey powder was 43 tons on the square inch (6554 atmospheres), The capacity for performing work of the various descriptions of powder was also found to be not very different, a similarity of result the more remarkable seeing that with, at all events, three of the powders there are striking differences both in their composition and in the decomposition they experience, and when in consequence material variations both in pressures at different densities and in potential energy might have been expected. With respect to the great difference in heat evolved by the Spanish and mining powders, it appears difficult to resist the conclusion that the small number of units of heat evolved by the latter is in great measure due to the quantity of heat absorbed in placing the very much larger proportion of the products of combustion in the form of permanent gases. This suggestion would also appear fully to explain the fact alluded to in the authors' first memoir, and to which they had been led purely by experiment, namely, "that the variations observed in the decomposition of gunpowder do not, even when very considerable, materially affect either its tension or capacity for performing work." A comparison between different gunpowders, or a comparison between gunpowder and other explosive agents, cannot therefore, as has been proposed, be determined by a simple measurement of the corresponding units of heat they evolve. As regards the *actual* temperature of explosion, the results of the further experiments detailed in this paper leave little doubt that the temperature named in the authors' first memoir, viz., 2200° C., is not far removed from the truth for the principal powders with which they then experimented. The authors then discuss the constants in the equation, expressing the relation between the tension of the products of explosion and the volume these products occupy, as stated by them in their first memoir, and give values of those constants, corrected from the analyses and experiments made since the publication of that memoir, concluding their remarks upon this part of their subject with a table which gives in terms of the mean density of the powder products the tensions which would exist in the bores of guns were perfectly dry powder of normal composition suffered to expand, with or without production of work. The tensions are expressed in kilogrammes per square centimetre, tons per square inch, and atmospheres. The authors call attention to the great utility of a table (XI.) showing the theoretic work which a charge of gunpowder is capable of effecting in expanding to any value, v. The table given by them exhibits the theoretic work for all necessary volumes of v, from $v=1$ to $v=50$. The authors then discuss the causes which in the bore of a gun affect the energy realised by gunpowder, and point out that this energy varies very much with the powders employed, being in this respect dependent upon circumstances, such as the density of the powder, its size of grain, the amount of moisture, chemical composition, nature of charcoal used, &c., but that the energy may also vary considerably, even with the same powder, if the charges be not fired under precisely the same circumstances. For example, especially with slow burning powders, the weight of the shot fired exerts a very material influence upon the factor of effect, and the reason is obvious—the slower the shot moves at first, the earlier in its passage up the bore is the charge entirely consumed, and the higher is the energy realised. The same effect, unless modified by other circumstances, is produced when the charge is increased with the same weight of projectile. In this case the projectile has to traverse a greater length of bore before the same relief due to expansion is attained. The higher pressures which consequently rule react upon the rate of combustion of the powder, and again a somewhat higher energy is obtained. Both these increased effects, of course, correspond to an increased initial tension of the powder-gases, but, especially with the smaller guns, a very great difference in the realised energy may arise from other causes. The authors draw attention to the effect of retaining the shot in its seat for a greater or less time, thus giving rise in the former case to a more perfect and earlier combustion of the charge. They cite experiments to show that from this cause alone differences of energy in guns having a calibre of 12 centims. of 13 to 14 per cent have been obtained. It is pointed out that from the principles laid down it is possible, if the maximum chamber-pressure be known, to fix, very approximately, the position of the shot in the bore when the combustion of the charge may practically be considered to be effected. The authors have little doubt that the main theories upon which they insist, confirmed as they are by experiments made or facts obtained, under very great variety of circumstances, may be accepted as, at any rate, close approximations to the truth. It is satisfactory to find that the laws which rule the tensions and temperatures of gases under ordinary circumstances do not lose their physical significance, but are still approximately applicable, at the high temperatures and pressures they have been considering. At all events, it appears certain that the rules and tables they have laid down, as based on their analyses, experiments, and calculations, may for all practical purposes be accepted as correct, and may, bearing in mind the restrictions to which they refer in this memoir, be applied to nearly every question of Internal Ballistics.

June 19.—" On the Production of Coloured Spectra by Light," by Capt. Abney, R.E., F.R.S. Last year I incidentally mentioned in a note to the Royal Society (" Proceedings," vol. xxviii., p. 291) that the production of natural colours by the agency of light, examples of which were shown by Becquerel, was probably caused by the oxidation of silver compounds employed. I have ventured to return to the subject, in order to show that the colours are so produced and are not due to interference. I have sent, for the Society's inspection, pictures of the solar spectrum on silver plates, and also on compounds of silver held *in situ* by collodion. It will be observed that the spectrum has imprinted itself in approximately its natural colours; that on the silver plates it is more brilliant than on the collodion film, but that in the latter the colours are seen by transmitted as well as by reflected light. I reserve for the present the exact details of the production of these pictures, but may say that they are produced by oxidation of silver compounds when placed in the spectrum, an exposure of two minutes being amply sufficient with a wide slit to impress the colours. The colouring-matter seems to be due to a mixture of two different sizes of molecules of the same chemical composition, one of which absorbs at the blue end and the other at the red end of the spectrum, and the sizes of these molecules are unalterable whilst exposed to the same wavelengths as those by which they were produced. I believe it possible and probable that the colours may be preserved unchanged when exposed to ordinary daylight.

METEOROLOGICAL SOCIETY, *June* 18.—Mr. C. Greaves, F.G.S., President, in the chair.

"Thermometer Exposure—Wall *versus* Stevenson's Screens," by William Marriott, F.M.S. It being the practice of some observers to expose their thermometers on walls facing north, it seemed a suitable object of inquiry whether instruments so placed gave results comparable with those obtained from thermometers in a Stevenson stand in the open. A pair of Meteorological Office wall-screens were fixed to the brick wall of an outhouse with a northern aspect, so that the screens were in the shade except in the morning and afternoon of the summer months. The Stevenson screen was on a grass plot 17 feet square, and about 50 feet north of the wall-screen. The Paper contains the results of the comparison of the maximum and minimum temperatures in the wall-screen with those in the Stevenson stand for the twelve months ending 31st March, 1879. The figures show that the mean daily maximum temperature on the wall is below that in the open, the monthly differences varying from 0·0° to −2·1°, that for the twelve months being −1·0.° The minimum temperature on the wall was mostly higher than in the Stevenson stand, the differences varying from −0·1° to +1·3, the mean for the year being +0·5°. The individual differences, however, are sometimes much greater, the maximum temperature on the wall being considerably lower than that in the stand. For instance, the difference exceeded 4·0 five times in September and four times in March, the greatest being 6·7°: these extremes occurred on fine calm days. The minimum temperature on the wall was more than 2·0° higher than that in the Stevenson stand on five occasions in June, seven in July. and four in September. The mean daily range of temperature on the wall for the twelve months was 1·4° less than in the stand in the open. The greatest difference was on March 9th, when the range on the wall was 8·5 less than on the stand. These results seem to show that, although the mean temperature may be roughly ascertained from thermometers shaded by a wall with a northern aspect, this method of exposure affords less sensitive indications than those obtained from instruments in a properly exposed Stevenson stand.

PHYSICAL SOCIETY, *May* 24, 1879.—Prof. W. G. Adams, President, in the chair.

Mr. W. J. Wilson exhibited a new harmonograph and figures drawn by it. The figures drawn by prior harmonographs are all more or less imperfect, owing to loss of motion in the pendulums actuating the marking pen; and Mr. Wilson therefore designed a new instrument, which not only gives perfect figures, but admits of the phase of either of the two compounded motions being decreased by a known amount. In this instrument toothed wheels take the place of pendulums, the ratio of the teeth giving the ratio of the periods of the motions. By employing the device of an intermediate wheel gearing with two others, and arranging two or more wheels on the intermediate axle, a great variety of phase can be obtained for each motion. An ingenious adjustment by means of a movable nut allows the phase of either or both motions to be altered to a known extent, and thus an endless variety of figures can be obtained. As in other harmonographs, a writing table, on which the paper is placed, and an aniline glass pen, are used. Several of the figures done also on glass were thrown on the screen, the stereoscopic effect being very apparent. In reply to a query, Mr. Wilson said that he had adapted some of the figures to the stereoscope.

Prof. Hughes explained his new Induction Balance, and showed some of its principal effects. It is well known that on an electric current passing along a wire adjacent to another wire, an induced current is set up in the second wire in an opposite direction to the first or primary current. In the induction balance two primary circuits or coils are taken, with the same current (interrupted by a microphone acted upon by the ticking of a clock) running through both; and between these is placed a secondary circuit or coil in connection with a telephone. The primary coils are so wound as to oppose each other's induction on the intermediate secondary. There is a point, moreover, between where these opposed inductive influences exactly neutralise each other. If the secondary coil be placed there no induced ticking of the clock can be heard; but if the secondary be displaced to one or other side of this point the ticking can be heard in the telephone, increasing in loudness as the secondary approaches one or other of the primaries. If the distance between the primaries be graduated into a scale this contrivance becomes a sonometer, since it gives an absolute zero of sound and degrees of loudness. It is well adapted for measuring the hearing power of the ear, and when used for this purpose it is known as the audiometer. By splitting the secondary coil into two parts and placing each close to its proper primary, so that there are four coils in all arranged in two pairs, the extremes in one primary circuit and the means in one secondary, the two opposing parts of the balance can be separated from each other, so as not to disturb each other's action, and the balance made very sensitive by the closeness of the primaries and secondaries. Prof. Hughes finds that there is a line or zone of maximum induction midway between each primary and its secondary. If a conductor, such as a piece of metal, be put in this position it has a maximum distributing effect on the balance, due probably to the electric currents generated in it by the induction. The effect is apparently proportional to the conductivity of the metal. It requires an exactly similar piece of metal put between the other pair of coils to restore the equillibrium of the balance. A very slight difference of alloy or of weight between two like coins is at once observed from the imperfect restitution of the balance; base coins are thus also at once detected. Again, it is possible for a person to tell what particular coin or coins are in one part of the balance by trial of the same coins in his part. When plates of non-magnetic metals are held vertical in the balance their distributing effect is nil; iron, on the other hand, gives its maximum effect at this position, because its maximum effect overbalances its electrical effect. Two pieces of iron may therefore neutralise each other as cores in an induction coil. Steel is easy to balance compared with soft iron. Zinc disturbs most when placed along the sides of each pair of coils; iron when in centre. A certain length of metal laid along the outsides of the coils produces silence. The maximum line of inductive force is midway between the coils, and there is a line of minimum force at half the thickness of each coil; a metal placed at these lines of minimum force has no disturbing effect on the balance. Pressure, torsion, heat, magnetism, strain, and in fact all imponderable forces, are indicated, and their effects may be measured.

Prof. W. G. Adams believed that one result of Prof. Hughes's experiments will be the determination of the way in which the law of electro-dynamic induction depends on density. He also imagined that the metal when in the maximum line between the coils gathered the lines of force to itself, whereas when on the minimum lines it could not thus divert them.

Prof. Ayrton cited the early experiment of Faraday with a sheet of copper oscillating to rest between two opposite magnetic poles. The copper took a long time to stop; but a sheet of iron placed between two like poles was soon stopped, owing to its becoming imbued with an opposite polarity, and deflecting the lines of force. He also suggested that the divergence of the results for conductivity of metals got by the induction balance from those got by the Wheatstone balance might be due to that electric inertia suspected by Sir William Thomson.

Prof. Guthrie thought that the induction balance pointed to the conclusion that the disturbing effect of a conducting mass applied in this way is proportional to the quantity of electricity generated in it under certain conditions of temperature, &c. The determination of the conductivity of liquids would be a useful application of the balance.

Mr. Chandler Roberts gave some results which he had obtained from an examination of certain alloys by means of the induction balance. He had been able to detect a difference of 1 part in 1000 in the amount of silver present in two shillings of equal weight. He also pointed out that Matthiessen divided alloys into three classes:—(1) Solidified solutions of one metal in another; (2) Solidified solutions of one metal in an allotropic modification of another metal; (3) Solidified solutions of allotropic modifications of both metals. For the first class the curve of electric conductivity is a straight line; for the second, a parabolic curve; for the third, a bent line. Mr. Roberts found that the balance gave the characteristic curve for the first class with an alloy of lead and tin, and for the second with an alloy of gold and silver. With a copper-tin alloy, which is a good example of the third class, he found the curve given by the balance to be intermediate between Alfred Rich's curve of density and Matthiessen's curve of conductivity, and considers that the balance is influenced by the density as well as by the conductivity of the metal interposed.

Prof. Hughes said that as the working of metals appeared to effect their conductivity, he was inclined to rely more on the conductivity measurements of the balance than on those of the Wheatstone bridge. By the balance plain pieces of metal were taken, whereas by the bridge wires were mostly taken. He would rather not give any theories yet as to the results obtained from the balance.

Dr. Erck then exhibited his novel pump for lifting solutions out of batteries. It consists of a closed vessel, funnel-like, the stem dipping into mercury, a column of which ascends the latter to a certain height. Two tubes emerge from the cover, one dipping into the liquid, the other opening to the air. By altering the pressure inside the vessel the solution rises to the latter, and can escape from it by trickling through the mercury.

NOTES.

BIOLOGY.

SOME interesting results on the hereditary transmission of artificial injuries have been obtained by M. Brown-Séquard. He concludes that the young of parents abnormally constituted inherit external lesions, but not the central anomaly which determines such lesions.

M. Geoffroy has recently discussed the views of Magnus on the evolution of the colour-sense. He agrees substantially with Mr. A. R. Wallace and Mr. Grant Allen that it was not the perception of colours which was wanting in Homer's days, but merely a precise nomenclature for shades.

M. Zolyet has laid before the Society of Natural Sciences, at Bordeaux, some researches on the respiration of fishes. He finds that of all animals their respiration is the least active; they never, however, absorb more carbonic acid than they give off oxygen. Between the temperatures of 2° and 30°, which may be taken as the extreme limits of the surrounding medium compatible with life, the amount of oxygen varies from 1 to 10.

The action of the sulphide of carbon upon the vine has been examined by M. Boiteau. He finds that all doses destructive to the *Phylloxera*, from 6 or 7 grms. up to 10 grms. per hole,—*i.e.*, 12 to 24 grms. per square metre,—are injurious to the roots within a certain radius.

M. G. Pouchet states that Averrhoes is the first writer who gives an approximately true account of the sensation caused by the touch of electrical fishes. He compares it to magnetism, whilst Galen and others had considered it analogous to cold.

It is asserted by some bird-fanciers that the feathers of canaries may be turned red by feeding them, before and during the moulting season, with a mixture of hard-boiled egg and bread, dusted over with the best quality of capsicum in fine powder.

The "Revue Industrielle," after stating the conflicting views of Vegetarians and of the friends of a mixed diet, thinks that sufficient attention is not paid in this controversy to differences of race, temperament, climate, and employment.

M. Richet has investigated the effects of heat on the functions of the nerve-centres of the crayfish. Nervous voluntary action is weakened at 23° to 24°, and disappears completely at 26°. Reflex nerve action disappears at 27° to 29°, at 30° the respiratory flabellum ceases to act, and from 32° to 34° the motor nerves become inactive. It is remarked that amongst the Vertebrates the different functions of the nervous system are affected in a progression very similar to that traced among the Crustaceans.

According to "Le Journal d'Hygiène" the heron has on its breast large greasy tufts, which secrete a whitish unctuous matter of a disgusting odour, but which has a remarkable power of attracting trout, and probably other fishes. M. Noury, on placing the breast of a heron in a net, has invariably found it filled with trout.

With regard to the chemical structure of bones, Dr. Carl Aeby points out that the hygroscopic properties and the relations of density and volume, both among the different bones of a skeleton and among the strata of one and the same bone, show greater discrepancies among themselves than is often the case with the bones of entire genera. He considers these differences as points of departure for further research, the significance of which extends far beyond the sphere of physiology.

The larva of a Dipterous insect, probably belonging to the genus *Syrphus*, has been observed to devour the *Phylloxera* with great eagerness.

M. Ch. Chamberland shows that the germs or spores of *Bacillus subtilis* in neutral liquids resist a temperature of 100° C. for several hours, and multiply rapidly at 50°.

The idea prevails in various parts of France that it is impracticable to strip the bark of trees if a flock of sheep is passing to the windward.

According to M. J. G. Lemmon the age of the *Sequoias* of California has been greatly over-rated, none of them, according to the number of their annual rings, exceeding fifteen hundred years.

Prof. Gunning declares that recent experiments have much shaken his faith in the supposed destructive action of alcohol upon low forms of organic life. There is further no *a priori* reason to be urged in favour of the assumption that alcohol is fatal to bacteria and their germs. Its dehydrating power cannot have such an effect, since such low organisms are merely thrown into a latent condition when temporarily deprived of water.

M. Coursserrant, in a communication made to the Academy of Sciences, recommends the systematic use of coloured glasses in cases of colour-blindness. In experiments which he has undertaken he finds that on looking through a green glass at a sheet of white paper, upon which was pasted a narrow stripe of cherry-red paper, the latter not merely appeared more brilliant, but a rose tint was spread over the whole white surface. According to certain experiments made upon himself, the luminous sensation is still further heightened if a feeble constant current —say of 2 or 3 Gaiffe's elements—is passed through the retina at the moment of the production of the phenomenon.

The Report of the Inspector under the Vivisection Act for the year 1878 has made its appearance. The greatest number of licenses in force at any one time during the year was 45: of these 18 remained a dead letter. Under the remaining 27 about 481 experiments were performed, of which not more than 40 involved the infliction of serious pain. In Ireland 10 licenses have been issued, only 5 of which have been acted on, and no pain has been inflicted even in one of the 24 experiments performed. These statistics are surely enough to show the utterly farcical character of the anti-vivisection agitation which gave birth to the Act in question. Even if vivisection were a demonstrated evil, biologists might have been protected by the maxim "De minimis non curat lex."

Writing on the physical conditions of consciousness, Prof. A. Herzen says that a psychical act considered objectively is a molecular movement, *sui generis*, which an external impression conveyed from the afferent nerves to a reflex sensation induces in the central nervous elements; it is not yet psychic until the vibrations have reached a cellule of the grey substance, and it is no longer psychical from the moment when the vibrations cease or leave the central cell to communicate themselves to the afferent nerves to be expended in the form of muscular motion. The phenomenon, taken as a whole, presents two phases: in the first there is decomposition of the substance of the nervous elements, and a release of the latent energy which has been shut up; in the second there is a re-composition of their substance, and a storing up of latent energy, destined to serve for future expenditure. The author calls the first phase nervopsychic disintegration, and the second nervopsychic redintegration. He then proposes the following physical law of consciousness:—Consciousness never accompanies the integration or redintegration of the nervous elements; it accompanies merely their functional disintegration. Its intensity is simultaneously in direct proportion to the intensity of the disintegration, and in inverse proportion to the ease and the rapidity with which the internal work of each nervous element is discharged upon another sensitive or motor element, whether central or peripheric.

M. E. Heckel has presented to the Academy of Sciences an account of the action of the salts of strychnin upon the Gasteropod mollusks. Noting the fact that the alkaloids occur more frequently and possess a more powerful physiological activity the higher we ascend in the scale of vegetable life, he asks whether their special task may not be to defend the plant against animal enemies, especially as the more important an organ the more generally it is possessed of poisonous properties. He therefore finds it important to ascertain the action of the best known alkaloids upon certain selected terms in the animal series. For this purpose he has experimented with strychnin sulphate and oxalate upon *Helix pomatia* and *aspersa*, and *Zonites algirus*.

He observed that whilst *Helix aspersa*, of the mean weight of 6 to 6·70 grms., succumbed to a dose of strychnin sulphate or 0·025 grm., *Zonites algirus* of 8 grms. and *Helix pomatia* of 9 70 grms. perfectly resisted doses of 0·045 grm. He concludes that the Gasteropods enjoy a remarkable immunity from the effects of strychnin: that with them, as in case of the Vertebrata, the degree of hurtfulness of the poison is inversely as the weight of the animal, and that upon them as upon higher animals strychnin is tetanising.

CHEMISTRY AND TECHNOLOGY.

A writer in "Reimann's Färber Zeitung" points out that tartar emetic, as used in cotton-dyeing, serves not to fix the aniline colours themselves, but merely to fasten the tannin, thus playing the part of an indirect mordant. Several experiments have been instituted at the Färber Akademie in order to ascertain whether this application of antimony can be pronounced injurious to public health. It was found that water, in which cotton yarns dyed with aniline colours on a mordant of tannin and tartar emetic had been steeped, or especially boiled, gave distinct indications of antimony when tested in ordinary manners. The quantity of the metallic compound fixed on the fibre seems, indeed, far too trifling to have any effect upon human health. Still, in view of the panic which has seized upon the public mind, and of the tendency of the literary and political press to attribute all mysterious attacks of sickness to the influence of poisonous dyes, &c., Dr. Reimann—in our opinion very judiciously—advises dyers to dispense with the use of antimony, which will doubtless be found by no means necessary.

The effects of inhaling oil of turpentine have been described to the Academy of Sciences by M. Poincaré. He has examined 282 workmen who use this oil in their trades, and has kept animals for several months in a medium strongly charged with its vapour. Among the workmen the symptoms were headache, dizziness, watery eyes, weakness of sight, especially in artificial light, cough, and troubled digestion. In most cases the constitution became habituated to this agent, but sometimes a change of employment was necessary. The animals experimented upon remained in a normal condition if good ventilation was maintained. In confined air death ensued in consequence of congestion of the brain, lungs, &c.

PHYSICS.

Dr. Henry Draper, the eminent American physicist who is now on a visit to this country, has addressed recent meetings of the Astronomical and Physical Societies on his alleged discovery of oxygen in the sun by bright lines in the solar spectrum. He said that hitherto he had not been able to find these lines projecting from the limb of the sun like hydrogen, and his impression is that oxygen resides lower than the reversing layer. He had lately been extending the dispersion of the spectrum of terrestrial oxygen, and from a light of maximum intensity of one-candle power had now got a dispersion of 80 inches from A to O. He exhibited two of the original negatives of the solar spectrum, showing the bright lines. "The Times" gives some interesting particulars respecting the time and labour involved in this research. The bobbin of the Gramme machine revolves once for each spark used in obtaining the photographs of the air spectrum. Each photograph requires 30,000 sparks, and the photographs obtained in the last three years have required no less than 20,000,000 sparks, so that the bobbin of the Gramme machine has revolved 20,000,000 times. Although the petroleum engine used for driving the machine consumes only a couple of drops of oil at each stroke, 150 gallons have been used up in three years.

At the recent Soirée of the Royal Society several novelties of great scientific interest were exhibited. In the first room were exhibited Mr. Crookes's Exhausted Tubes and Apparatus, illustrating various phenomena connected with Molecular Physics in High Vacua. A description of these experiments will be found in the "Monthly Journal of Science" for June. In another room Prof. Guthrie, F.R.S., exhibited Broken Glass in Frames, illustrating the Fracture of Colloids. The working of the Writing Telegraph was shown by Mr. E. A. Cowper. The object attained by this instrument is that it enables the operator to write at a distant station many miles away, just as though he were present there himself, without requiring the use of any special signals, codes, or signs (to spell each letter as is now the practice), and without the assistance of any person to translate the signals as received. The instrument acts upon the simple principle of communicating at all times to the writing pen at the receiving end of the line, the exact position of the pencil of the operator at the sending station through two line wires, or, so to speak, giving the latitude and longitude of the pencil continually, the position of the pencil vertically being communicated by one wire, and the position horizontally being communicated by the other wire. The pencil of the operator has two light "contact rods" jointed to it, and one of these slides over the edges of a series of "contact plates," having various resistances interposed between them and the line wire, while the other rod slides over a second set of such plates connected to the other line wire; at the receiving end of the line each of these wires actuates its own needle. The two needles (which are placed at right angles to each other, and are provided with light springs) actuate one writing pen, so that the pen moves up or down, and backwards or forwards, in exact obedience to the motions of the pencil in the hand of the operator at the distant station. Both the paper written upon in pencil by the operator at the sending station and that written upon in ink by him at the receiving station move along as the writing proceeds, and the messages have only to be cut off from time to time, wound round a piece of card, and sent out to their destination, or put into an envelope and dispatched. Edison's Loud-speaking Telephone was exhibited by Mr. Arnold White and Mr. C. P. Edison. This instrument far surpasses every other form of telephone at present invented. It is thus described by Mr. Conrad Cooke. A cylinder composed of precipitated chalk, to which a small proportion of acetate of mercury is added, the whole being moistened into a saturated solution of caustic potash and moulded into a cylindrical form by being subjected to hydraulic pressure, is mounted upon a horizontal axis, and is made by suitable mechanism to revolve beneath a metallic strip, which is maintained with a uniform pressure against the surface of the chalk. At the point where the strip rests upon the cylinder a small plate of platinum is fastened, and the opposite end of the strip is attached to the centre of a diaphragm of mica, four inches in diameter, firmly fixed to the framing of the instrument by its circumference. By connecting the strip to the zinc element of a voltaic battery, and the chalk cylinder to the copper pole, and rotating the cylinder at a uniform speed away from the diaphragm, it will be found that, when no current is passing, the friction between the moistened surface of the chalk and the platinum strip is sufficient to drag the centre of the diaphragm inwards, and it will take up a fixed position of equilibrium when the frictional pull in the centre of the diaphragm is equal to the elastic tension of the strained diaphragm. The moment, however, that an electric current is allowed to pass between the strip and the cylinder, electro-chemical decomposition takes place, the friction between them is reduced, and the diaphragm, finding its elastic tension unopposed, flies back to a second position of equilibrium dependent upon similar conditions; and, if a variable or undulatory current of electricity be transmitted through the instrument, the diaphragm will be kept in continual motion by the constantly varying friction existing between the chalk and the platinum dragging the diaphragm in opposition to its own constant elastic tension. So marvellously sensitive is this simple mechanical arrangement to the smallest as well as to the most rapid and complicated variations in electrical intensity taking place in the transmitted current, that all the complex sonorous undulations propagated by human articulation instantly produce their corresponding variations of frictional resistance, and the diaphragm reproduces, in a loud voice, the words which are being uttered into the telephone at the distant station. The cause of the superiority in power of this instrument over others must be looked for in the fact that the vibration of the diaphragm is produced, not as in other telephones by an electric current, but by local mechanical means, that is to say, by the rotation of the cylinder by hand, by clock-work, or by any other motive power. The function of the electric current is purely a controlling and regulating one; it determines how much or how little of the force exerted on the spindle is to be communicated to the diaphragm, and the time when that force is to be brought into play, and it may, mechanically speaking, be compared to a frictional clutch, coupling a machine to a steam-engine, and which, at any moment, may be made to transmit the full power of the motor to the machine, or, by reducing the friction, to transmit only a portion of that power.

The Parliamentary Committee on Electric Lighting have concluded their labours and issued their report. We shall refer to the evidence in a future number of this journal. In their report the Committee state that the general nature of the electric light has been well explained in the evidence of Prof. Tyndall, Sir William Thomson, Dr. Siemens, Dr. Hopkinson, and others. It is an evolution of scientific discovery which has been in active progress during the whole of this century. Essentially the electric light is produced by the transformation of energy either through chemical or mechanical means. The energy may be derived from a natural force, as, for instance, a waterfall, or through combustion of a material in the cells of a voltaic battery, or of fuel in a furnace. The energy being converted into an electric current, may be used to manifest electric light by passing between carbon points, or by rendering incandescent solid bodies, such as iridium. A remarkable feature of the electric light is, that it produces a transformation of energy in a singularly complete manner. Thus the energy of one-horse power may be converted into gaslight, and yields a luminosity equal to 12-candle power. But the same amount of energy transformed into electric light produces 1600-candle power. It is not therefore surprising that while many practical witnesses see serious difficulties in the speedy adaptation of the electric light to useful purposes of illumination, the scientific witnesses see in this economy of force the means of great industrial development, and believe that in the future it is destined to take a leading part in public and private illumination. There is one point on which all witnesses concurred, that its use would produce little of that vitiated air which is largely formed by the products of combustion of ordinary illuminants. Scientific witnesses also considered that in the future the electric current might be extensively used to transmit power as well as light to considerable distances, so that the power applied to mechanical purposes during the day might be made available for light during the night. So far as the practical application of the electric light has already gone there seems to be no reason to doubt that it has established itself for lighthouse illumination, and is fitted to illumine large symmetrical places, such as squares, public halls, railway stations, and workshops. It is used in Paris for lighting shops which require a light by which different colours may be distinguished, and has recently been used in England for the same purpose with satisfactory results.